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COMPARATIVE OBSERVATIONS ON THE INDIRECT COLOR RANGE OF CHILDREN, ADULTS, AND ADULTS TRAINED IN COLOR.¹

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The work already done in indirect color vision has been largely summed up in the original investigations of A. Kirschmann⁽¹⁾², A. E. Fick⁽²⁾, C. Hess⁽³⁾ and A. Fick.⁽⁴⁾

Kirschmann, with others, has shown that only a small part of the retinal surface is sensitive to the fundamental color impressions; that the retina is sensitive to impressions of blue much farther from the centre than to any other color impression, or giving the eccentric range of the different colors, he finds (beginning with the color having the greatest range and proceeding toward the centre) blue, yellow, orange, red, green, purple, violet.

He finds, further, that "The action of the peripheral retina varies greatly, in different directions, from the centre, and that the ranges of all colors extend farther in the nasal and

¹This investigation was made in the psychological laboratory of the Leland Stanford, Jr., University, and the writer desires in this connection to thank Dr. Frank Angell and Prof. Earl Barnes for helpful suggestions and criticisms, and also Mr. J. C. Hammel, and others, for kind assistance in the laboratory.

²The small figures in parenthesis refer to the bibliography at the end of the article.

upper parts than in the lower and temporal parts of the retina." He also finds that the size of the colored objects has much to do with the extent of the range.

A. E. Fick finds, in going over an investigation of Charpentier,¹ that there is considerable mutual assistance of separate neighboring retinal spots in the perception of color and of light. He also finds that the centre retina is more sensitive to color and form impressions, while the eccentric retina, as we pass toward the periphery, is more sensitive to light and motion impressions, but much faster to some colors than to others; and that intensity has influence on the perception of light, color, form and number, but just how great is not determined.

The paper of Hess, published in 1889, is a very careful re-investigation of peripheral color vision, both spectral and pigment colors having been used, and its results partly support and partly supersede those of previous experimenters. He finds equal color ranges for red and green, and for blue and yellow, when these colors are of the hue of Hering's original colors (*Urfarben*).

A. Fick's study, to which reference has been made above, concerns itself with the theory of color vision, and only touches indirectly upon the work which the writer has in hand.

The present investigation has been like that of Kirschmann, in being a study of the eccentric color range, but different, in being a comparative study of the eccentric color range of individuals differing in age, sex and previous color training.

After nearly two months of preliminary testing of both the perimeter and a flat surface apparatus (campimeter), it was concluded that the former possessed more advantages, with fewer disadvantages, and for this reason all the results recorded in this paper, excepting those on the upper and lower vertical meridians, were made on the perimeter. In the writer's opinion the errors arising from unequal illumination in different parts of the perimeter² are more than counterbalanced by the variation in angular magnitude in the case of the campimeter, especially when, as in the case of these experiments, the perimeter was illuminated equally from the sides, from behind and from a skylight; all windows being slightly to the rear of the observer. In fact, the results obtained on either apparatus were almost identical up to about 30° from the fixation point, but for greater distances the results given by the

¹Arch. d'opt. Juli-August, 1886.

²Kirschmann(¹) S. 600.

campimeter were from 1° to 15° less than those given by the perimeter, varying with the angle. It is difficult to see how accurate results can be obtained on the campimeter, when the angular magnitude is greater than 30° until we know more definitely the effect of size and intensity on color vision.

The only meridians in which the illumination of the perimeter was thought to vary were the upper and lower vertical meridians, and as the color range in these is small, the campimeter was used for them.

The perimeter apparatus consisted of a darkened steel rod, made in the form of a half-circle, fastened to a black wall by a screw at its middle point. This rod could therefore be adjusted in any desired meridian, the screw on which the perimeter turned being the point on which the uncovered eye of the observer was fixed. The observer was seated in an adjustable chair which carried a head rest. In this latter his head was placed, with his eye on a level with, and just two feet away from the fixed point, or at the centre of the curve of the perimeter. The only difference between the campimeter and the perimeter above described was the substitution of a dark rule, eight feet long, for the semi-circular rod. This was fastened to the wall in the same way and graduated to inches and projected degrees.

The colors used consisted of Bradley & Co.'s colored cylinders (familiarily known as the Hailmann beads). The cylinders were 12 mm. long and 12 mm. in diameter, and were perforated with a small opening from end to end, by means of which they were easily fixed on the end of a darkened steel rod 2 mm. in diameter and 1 m. long, used for moving them along the perimeter. The colors were first brought into the field of vision from the periphery by the operator, and the exact place on the perimeter noted at which the color could be recognized by indirect vision. The color was then placed at the centre and gradually moved toward the periphery until it lost all color, all changes in color, and the places at which they occurred, being also noted. One eye of the observer was continually covered, while the other was kept constantly on the fixed point during the time the color was moving to or from the centre. The latter was permitted to turn about, however, during the time the experimenter was recording the past result or was adjusting a new color. Six colors were used—red, orange, yellow, green, blue and violet; more colors became confusing and were not named with accuracy. Purple and violet could not be accurately distinguished in indirect vision even by those most practiced in color discrimination, *e. g.*, teachers

of painting. Two persons who had had considerable experience as art teachers said they could not tell, as violet was being brought toward the centre, whether it was going to be violet or purple, and were only sure when it was within a few degrees of the centre. Possibly this may be accounted for by the fact that we have little or no experience in color discrimination in the peripheral parts of the retina, and color sensations there are not only different from those at the centre, but have also not become grouped or classified. The above is only true, however, of tints and shades of the same color or of neighboring color mixtures, for, as Kirschmann⁽¹⁾ has observed, a spectral red, and a red carrying a little blue become noticeably different as they pass toward the periphery, the former changing to orange or yellow, the latter to blue. The same may be said of the other colors.

The colors used were supposed to be true imitations of the corresponding spectral colors, and were perhaps as exact imitations as it is possible for pigment colors to be. Fresh cylinders were used for each individual tested, so as to prevent all danger of change by use. Three double tests (*i. e.*, three tests with the color approaching the centre and three with color receding from the centre) were made with every color in every one of the eight meridians, and the average (range) of these tests taken as the one nearest correct.

The results obtained by the present investigation, in so far as they represent ground traversed before, agree with the results of Kirschmann⁽¹⁾, Fick⁽²⁾ (⁴), Raehlmann⁽⁵⁾, and others, in the following facts: (1) The ranges for the different colors were from 1° to 2° greater when the colors were moved from the centre toward the periphery than when they were moved from without toward the centre. (2) There are certain meridians in which the colors can be seen and recognized much further than in others. The upper nasal half of the retina is sensitive to colors farthest toward the periphery, while the under temporal part is sensitive to color over the least area. (3) The colors seem to arrange themselves in a certain definite order, according to the distance they can be seen in indirect vision. Beginning with the color having greatest range and proceeding toward the centre, we have first blue, then yellow, orange, red, green, violet. (4) The colors seem to fall into two rather noticeable groups—the blue-yellow group and the red-green group. Blue and yellow do not coincide in range but fall nearer together than in Kirschmann's investigation. The same is true of red and green. (5) As shown by Raehlmann⁽⁵⁾ all the colors used entered the field of color vision as either blue or yellow (*i. e.*, yellow or orange). Blue and yellow

give no sensation of color when seen beyond their range, unless different shades of gray may be called color. Green is generally seen first as yellow, and a few degrees nearer the centre is recognized as green. Red is generally seen first as yellow, then as orange, then as red, though often it is seen first as orange then as red. Orange is generally seen first as yellow, and violet is always seen first as blue.

The following facts may be inferred from other investigations, but I believe have not been stated: *e. g.*, blue is the most stable and permanent of all the colors. It is never mistaken for any other color, enters the sensitive color field as blue and remains a blue throughout the entire field. Green, although its range is very much less, comes next to blue in permanency; while it is generally seen outside of its range as yellow, yet there is quite sure to be found a certain definite place on every meridian within which the same observer is sure of the color. Yellow is perhaps the most variable: for example, in all the tests made on the same individual the range for blue never varied more than 3° , and the subject always felt sure when blue entered the field, whether he expected it or not. In green, the variation (with the same observer) was never beyond 4° , but he was not quite so sure as with blue. With yellow, on the contrary, the observer often varied from 1° to 10° , and never felt quite sure but that the yellow might prove to be orange, or red, or even green.

Another interesting fact is that violet is seen as blue a degree farther than the blue itself, but is not recognized as violet until within the range for green.

The variation between yellow and orange, as mentioned by Kirschmann¹ was not confirmed by the present investigation, as yellow showed a greater range than orange in every one of the eight meridians, and came as near to blue in one meridian as in another.

The purpose of the present investigation was, however, less to study the general subject of peripheral color vision than to answer the following questions:

Can children see colors in indirect vision as far as adults?

Does sex have anything to do with difference in the range?

What influence has color education on the range?

As this study was for the purpose of establishing the comparative range, great care was taken that all the conditions should remain as nearly constant as possible. All tests were made on uniformly clear days, between 2 and 4 P. M. As soon as the observer showed signs of fatigue or

¹S. 609.

inattention, the work ceased and was resumed at another time. The writer made all the tests himself (assisted by Mr. J. C. Hammel, a fellow student in psychology), in order that there might be uniformity in the manner of presenting the colors, although the observer was always kept ignorant of the order or of the color approaching. The difficulties which occur here are, for the most part, similar to those which occur in all experimental work in psychology and need not be mentioned. There was one difficulty, however, which at times became very annoying, especially in the work with children, and as the writer has failed to find it mentioned by others, a little space will be devoted to it here.

The eye of the observer is supposed to remain fixed on a small surface at the centre (5 mm. in diameter in these tests) during the time the colors are approaching or receding, but as the color enters the field of vision the attention is diverted from the fixation point to the approaching color, and as the attention becomes diverted the eye often unconsciously follows it a little. The same is true when the colors are receding. In this way it often occurs that the eye has changed from 1° to 10° , or even 15° in children, without the observer being conscious of the fact. Some way of overcoming the errors which would result from this change of vision is necessary, and as the observer is not conscious of the fact he cannot be trusted to tell when his eye has changed. To avoid the above error it was found necessary for the operator to watch constantly the eye of the observer and not to record any results in which the eye wavered from the fixation point. Such a control would obviously be inapplicable to the dark room experiments that others have preferred.

Now for the answers to the questions. First: Can children see color in indirect vision as far as adults? The writer first obtained the color range of ten adults who were students in the university, but found later that he could not get so many children of any one age, and selected from the adults six whose color range seemed to vary least. He then obtained the color range of six thirteen-year-olds, and next the color range of six children who were seven-year-olds.

In Tables I, II, III and IV are presented the results of these investigations. The figures indicate the number of degrees from the centre point at which the colored objects could be correctly seen, and represent the external visual field. When referred to the retina the meridians must, of course, be reversed, the outer horizontal meridian would, on the retina, be the *i. e.*, nasal meridian, etc., etc. The average results are shown by diagrams in the plates at the end of the article. These diagrams

TABLE I.

ADULTS. CLASS III.

SEVEN-YEAR-OLDS. CLASS I.										ADULTS. CLASS III.															
MALES.					FEMALES.					MALES.					FEMALES.										
A		B		C		D		E		F		A		B		C		D		E		F			
R	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L		
38	40	42	42	43	42	42	38	40	40	40	41	67	67	79	80	72	73	76	75	72	70	78	70	74	
Outer Horizontal,	34	34	32	37	38	33	37	35	35	33	35	62	62	70	70	70	70	67	68	65	66	70	64	67	
Outer Low. Obliq.,	25	24	28	28	25	25	28	27	25	27	23	24	25	40	40	45	45	40	38	42	42	40	38	38	
Lower Vertical,	25	25	26	24	25	25	28	27	25	25	25	41	42	36	38	40	40	36	36	38	38	36	38	37	
Inner Low. Obliq.,	25	25	26	24	25	25	28	27	25	25	25	41	42	36	38	40	40	36	36	38	38	36	38	37	
Inner Horizontal,	29	28	24	24	24	24	25	26	28	27	24	42	42	38	40	42	42	40	37	38	40	36	39	40	
Inner Up. Obliq.,	27	25	24	24	24	24	25	25	24	24	24	42	42	38	40	42	42	34	35	34	36	34	33	34	
Upper Vertical,	18	18	19	18	18	20	20	17	19	17	18	34	34	35	35	33	33	32	34	35	34	35	31	32	
Outer Up. Obliq.,	25	25	26	29	30	28	31	27	31	25	26	27	29	46	42	47	40	46	45	44	44	46	42	46	
Average Range,	26	28	28	29	29	29	29	29	29	29	28	47	47	48	48	47	47	47	46	46	47	44	45	46	
Sex Average,	28					28					47.4					47.4					45.7				

THIRTEEN-YEAR-OLDS. CLASS II.

THIRTEEN-YEAR-OLDS. CLASS II.																		ADULTS TRAINED IN COLOR. CLASS IV.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																								
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The External Visual Field for Blue.

The above represents the average results of three double tests of each individual through all the meridians as indicated. On this chart, in column eight, is also represented the average range for light and shade of each class. The letters, A, B, C, D, E, F, represent the different individuals of each class. R, right eye; L, left eye.

TABLE II.

SEVEN-YEAR-OLDS. CLASS I.										ADULTS. CLASS III.									
MALES.					FEMALES.					MALES.					FEMALES.				
A		B		C	D	E	F	Av. Range for every Meridian.		A		B		C	D	E	F	Av. Range for every Meridian.	
R	L	R	L	R	R	L	R	L	R	L	R	L	R	R	L	R	L	R	L
35	37	40	38	38	30	32	33	33	33	30	35	35	78	74	76	73	72	73	71
Outer Horizontal,																			
Outer Lower Oblique,	32	26	35	38	33	32	29	26	33	30	28	31	30	70	64	67	64	57	55
Lower Vertical,	21	20	27	27	23	20	25	25	20	21	20	20	23	42	42	38	43	35	38
Inner Lower Oblique,	24	21	22	22	24	22	23	24	20	25	24	19	23	22	30	36	40	40	37
Inner Horizontal,	28	29	25	22	26	22	20	21	24	27	24	24	21	24	42	42	40	37	38
Inner Upper Oblique,	25	25	19	21	18	19	20	18	16	15	16	16	20	23	33	36	40	40	37
Upper Vertical	14	14	18	18	16	16	16	16	15	15	16	16	23	33	36	40	40	37	38
Outer Upper Oblique,	23	25	24	27	19	20	17	27	25	21	19	21	23	46	45	47	45	43	45
Average Range,	25+	25	26	27	25	24	23	24	24	25	23	21	24	47	47	48	47	43	44
Sex Average,	25				23				46.3				44						

THIRTEEN-YEAR-OLDS. CLASS II.

THIRTEEN-YEAR-OLDS. CLASS II.										ADULTS TRAINED IN COLOR. CLASS IV.																	
MALES.					FEMALES.					MALES.					FEMALES.												
A		B		C		D		E		F		A		B		C		D		E		F		Av Range for every Meridian.			
R	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L
55	52	57	55	45	54	55	59	54	54	57	57	54	55	78	82	65	73	82	83	75	70	70	71	75	69	74	74
Outer Horizontal,																											
Outer Lower Oblique,	44	46	45	48	45	54	53	49	50	48	48	49	55	66	47	46	70	70	58	52	60	64	65	64	59	60	
Lower Vertical,	30	30	30	31	30	30	35	33	25	29	27	31	29	31	37	38	33	31	43	45	33	34	43	43	39	39	
Inner Lower Oblique,	34	28	28	30	35	30	28	30	25	25	31	26	30	28	35	32	36	35	35	35	32	39	32	43	35	34	
Inner Horizontal,	32	25	30	28	28	33	28	26	23	27	32	29	28	37	44	38	43	34	38	35	36	40	34	36	36	39	
Inner Upper Oblique,	25	26	25	26	30	30	28	25	27	23	26	28	27	33	35	38	38	35	37	32	33	41	42	34	36	37	
Upper Vertical,	20	22	18	23	22	20	26	25	21	20	23	26	22	23	35	32	34	30	27	33	30	30	31	30	31	31	
Outer Upper Oblique,	28	28	27	30	30	31	33	35	40	33	32	32	32	37	43	38	38	38	43	37	37	40	40	46	45	39	41
Average Range,	33	32	32	34	33	34	36	36	34	33	34	35	34	34	44	47	41	42	45	48	42	40	46	47	44	45	44
Sex Average,	33				34.5				43.4				44.5														

The External Visual Field for Yellow.

The above chart represents the average results of three double tests of each individual through all the meridians as indicated. The letters A, B, C, D, E, F, represent the different individuals of each class. R, right eye; L, left eye.

TABLE III.

TABLE III.

	SEVEN-YEAR-OLDS. CLASS I.										ADULTS. CLASS III.															
	MALES.					FEMALES.					MALES.					FEMALES.										
	A		B		C	D		E	F	Av. Range for every Meridian.	A		B		C	D		E	F	Av. Range for every Meridian.						
	R	L	R	L	R	R	L	R	L		R	L	R	L	R	R	L	R	L							
Outer Horizontal,	30	30	31	30	35	36	30	32	27	28	30	31	30	31	56	50	54	48	46	45	45	47	49	47		
Out. Lower Oblique,	26	26	26	27	30	30	29	27	29	25	25	27	27	30	30	37	38	34	35	30	34	33	32	33	34	
Lower Vertical,	19	20	20	20	17	17	21	21	18	19	19	19	19	20	20	23	20	23	25	26	20	22	22	23	23	
In. Lower Oblique,	19	18	18	17	18	17	20	16	20	22	17	18	18	23	20	24	25	22	25	24	26	24	24	23	24	
Inner Horizontal,	21	22	21	18	20	18	15	17	19	25	19	19	20	27	25	25	23	24	23	24	28	26	25	24	24	
In. Upper Oblique,	20	19	16	16	20	17	16	17	20	18	16	17	18	22	23	28	27	25	23	22	27	23	22	24	25	
Upper Vertical,	15	15	12	12	15	13	13	14	16	13	13	14	14	17	17	20	17	19	18	21	20	19	18	19	18	
Out. Upper Oblique,	17	20	16	18	22	21	16	18	22	23	21	19	19	20	25	27	25	28	24	23	27	28	23	24	25	
Average Range,	21	21	20	20	22	22	20	21	20	22	21	20	20	21	27	27	30	28	27	29	29	28	27	27	29	27
Sex Average,	20										20.5					27.8					27.2					

THIRTEEN-YEAR-OLDS. CLASS II.

	MALES.						FEMALES.						Av. Range for every Meridian.	
	A		B		C	D	E	F						
	R	L	R	L				R	L					
										R	L	R		L
Outer Horizontal,	33	35	35	36	33	35	31	37	39	35	34	35	34	36
Out. Lower Oblique,	30	27	31	26	32	29	31	30	32	30	32	32	31	29
Lower Vertical,	20	20	20	20	19	18	19	19	16	20	17	21	18	20
In. Lower Oblique,	20	18	18	19	21	20	20	19	20	17	22	21	20	19
Inner Horizontal,	22	20	20	21	20	20	21	20	20	17	22	21	20	19
In. Upper Oblique,	19	21	20	19	21	20	19	17	20	17	20	19	20	19
Upper Vertical,	15	16	15	16	17	18	17	16	17	15	16	16	16	16
Out. Upper Oblique,	21	20	20	21	22	19	22	20	21	23	22	24	21	23
Average Range,	23	22	22	22	23	22	23	22	23	22	23	24	23	23
Sex Average,	22						22.7							

The External Visual Field for Red.

The above chart represents the average results of three double tests of each individual through all the meridians as indicated. The letters A, B, C, D, E, F, represent the different individuals of each class. R, right eye; L, left eye.

TABLE IV.

SEVEN-YEAR-OLDS. CLASS I.												ADULTS. CLASS III.																							
MALES.						FEMALES.						MALES.						FEMALES.																	
A			B			C			D			E			F			A			B			C			D			E			F		
R	L		R	L		R	L		R	L		R	L		R	L		R	L		R	L		R	L		R	L		R	L				
23	27	25	27	25	25	24	28	21	23	20	20	23	25	43	42	44	40	37	37	44	42	40	43	42	43	43	41	42	41	42	41				
23	24	23	24	22	19	23	25	21	19	21	21	22	22	30	30	36	35	33	30	31	31	32	30	32	31	32	31	32	31	32	31				
14	14	15	17	15	15	18	18	16	16	17	17	16	16	20	20	25	23	20	23	23	26	20	25	22	20	22	23	26	20	25	22	23			
15	14	11	13	14	13	13	12	12	15	15	14	13	14	21	20	20	26	22	21	20	23	22	20	21	21	21	21	21	21	21	21	21			
17	18	13	18	15	15	14	14	15	18	15	13	15	16	23	20	20	22	23	23	25	25	23	25	21	23	25	23	25	23	25	23	23			
17	16	12	13	13	15	14	14	13	14	14	13	14	14	20	21	25	26	23	24	21	20	19	18	20	21	21	21	21	21	21	21	21			
11	11	11	12	12	12	11	11	12	13	11	13	11	12	19	18	18	19	17	16	21	20	18	20	18	19	18	18	19	18	19	18	19			
15	16	14	15	17	17	14	15	18	17	17	14	16	16	24	28	24	27	24	23	26	26	23	23	26	27	25	26	25	26	25	26	26			
17	17	15	17	17	16	16	16	17	16	16	16	17	17	25	25	27	27	25	25	26	27	24	25	25	25	25	25	25	25	25	25	25			
Sex Average,						17-	16.5						25.5						25.6																
Outer Horizontal,																																			
Outer Lower Oblique,																																			
Lower Vertical,																																			
Inner Lower Oblique,																																			
Inner Horizontal,																																			
Inner Upper Oblique,																																			
Upper Vertical,																																			
Outer Upper Oblique,																																			
Average Range,																																			

THIRTEEN-YEAR-OLDS. CLASS II.

	MALES.						FEMALES.						Av. Range for every Meridian.		Av. Range for every Meridian.														
	A			B			C			D			E			F		R	L										
	R	L		R	L		R	L		R	L		R	L		R	L												
Outer Horizontal,	32	31	35	32	33	33	33	33	32	38	35	27	31	33	32	37	40	31	32	43	38	33	34	40	40	30	32	36	36
Outer Lower Oblique,	27	27	28	28	28	25	31	28	30	28	25	29	28	28	26	32	26	27	35	28	28	30	33	33	30	27	30	30-	
Lower Vertical,	17	18	17	18	16	16	18	16	16	19	18	17	17	18	23	20	21	24	22	20	22	22	26	26	24	24	28	24+	
Inner Lower Oblique,	18	19	17	18	19	15	18	17	16	18	17	17	18	22	20	21	24	22	20	22	22	25	25	26	26	22	25	25	
Inner Horizontal,	18	20	17	19	17	19	20	18	20	18	20	18	17	18	26	30	27	26	24	23	24	25	24	27	23	25	25	26	
Inner Upper Oblique,	16	16	18	17	20	20	16	18	18	17	16	18	17	18	18	24	22	22	18	20	25	24	24	24	21	21	21	21	
Upper Vertical,	14	15	14	15	17	14	17	17	16	15	14	14	15	15	19	18	16	16	16	18	18	18	18	17	17	18	17	17	
Outer Upper Oblique,	19	18	17	20	16	17	20	16	17	17	19	20	17	18	17	22	25	26	22	21	21	23	27	22	22	21	22	22	
Average Range,	20	21	21	21	23	20	21	21	22	21	19	21	21	20	21	24	26	24	24	25	23	24	25	26	27	23	24	24	25
Sex Average,	20.5						20.7						24.5						24.5										

The External Visual Field for Green.

The above chart represents the average results of three double tests of each individual through all the meridians as indicated. The letters A, B, C, D, E, F, represent the different individuals of each class. R, right eye; L, left eye.

also represent the external visual field, and must be reversed in order to represent the retinal color fields. In the tables and diagrams the results of only four colors (R. Y. G. B.) have been represented, as these seemed sufficient to illustrate the point and less confusing than more colors would have been.

On examining these tables one is impressed with the general similarity of the fields in persons of different ages. The general form of the visual field is, in all cases, somewhat elliptical, but more circular in youth. The extent of the visual field is evidently larger in the adults, and so uniformly is this the case that it seems safe to answer the first question, "Can children see colors in indirect vision as far as adults?" in the negative.

Representing the average range for the adult eye for the four colors as 100, the average for the thirteen-year-olds would be represented by seventy-seven, and the seven-year-olds by sixty-one. This, expressed in retinal surface, would be in the ratios of 100, fifty-nine, and thirty-seven respectively. The greatest difference in the range was found with the blue and the least with the red. The order, beginning with the color showing the greatest difference and ending with the one showing the least difference, is blue, yellow, green, red. There is, however, one exception to this order in case of the seven-year-olds, where yellow presents a greater difference than even blue. (See table.) Perhaps this can be accounted for in the hesitancy of the children to speak as soon as they received a sensation of yellow. The observer was always requested to speak as soon as he received any sensation of color whether it should prove to be true to the color approaching or not, but the children soon learned that what seemed to be yellow, might, on approaching nearer, prove to be orange, red or green, and would often hesitate until they felt quite sure that it was yellow. The writer endeavored to overcome this cautiousness but was not entirely successful.

The question that now arises is: Why cannot children see color in indirect vision as far as adults? The discussion of this problem would require a paper in itself and must be deferred; however there are a number of points which present themselves. There are just as many degrees in the child's eye as in the adults, and so far as is definitely known, the retinal layer extends as far forward in the one as in the other. The difference in the convexity of the crystalline lens favors the child. We can hardly account for it through inattention, for the thirteen-year-olds seemed to be able to give as close attention to the approaching color as the adults, yet they

could recognize color only three-fourths as far. Again, the seven-year-olds could see objects 78% as far as adults, while they could see color only 61% as far. Although it is difficult to decide what the true cause is, yet the writer believes that the cause must be looked for in the order of the color development itself, whatever may be regarded as the true theory of color vision. Beginning with the visual color field, as seen by the adult, and passing backward toward and through the visual field of the child, we notice that the field not only decreases in extent but also becomes more circular. The same is true of the general visual field, *i. e.*, the visual field for light and shade, only the latter field decreases much less rapidly, or, in other words, shows a much greater proportional range in children than the color field. Now, should we continue still farther backward, and this law hold true, we should finally reach a point in the child's development where the eye ceased to be sensitive to color impressions; and from previous observations on young children, the writer would place this point not earlier than the fourth week of life, if as early as that. Perhaps no one will doubt the statement that the child becomes sensitive to light and shade much sooner than to color as color. A very young child may be attracted by a bright color, not on account of the color but on account of the light it contains. The above observations show that light and shade either develop faster in children than color, or else start sooner to develop, for while the range for light and shade of the seven-year-olds, as compared with adults, stands seventy-eight to 100, color, for the same individuals, stands as sixty-one to 100, linear measure. The writer very much questions whether his own children were able to perceive blue before they were eighteen months old, while they knew and seemed to enjoy red, orange and yellow very much earlier. If it be true that blue is the last of the colors to be perceived by the child, it becomes interesting to know why in after life blue has the greatest range of all the colors.

Judging from all the evidence at hand it seems probable that the child inherits from past generations an ever increasing color tendency, but nothing more, that he must come in contact with the real colors, or, in other words, the different color stimuli must play on the retina in order to develop this color tendency into a real mechanism for the discrimination of color, and that this mechanism begins to develop in earliest childhood, develops slowly and is finally completed in adult life. Further discussion of this point must be reserved for a future paper.

The second question was: Does sex have anything to do with difference in the range? There is a prevalent belief that woman has a better color eye than man, *i. e.*, greater inherent power of seeing and discriminating color, and that there are more color-blind men than women. The writer doubts, however, whether these beliefs are founded on facts. At least a carefully arranged and conducted experiment on 200 children (a number too small, perhaps, for accurate judgment) showed practically no sex differences in color vision. In the present experiment on visual range the data are entirely too limited to warrant any definite conclusions in the matter of sex, but the figures at least do not show that woman possesses any color superiority over man. In Tables I to IV, we find in Class III, designated adults, where there were three male and three female observers, that the average range for the males with blue was 47.4° , for the females, 45.7° ; the range of yellow, for males, 46.3° , for females 44° ; the range of red, for males, 27.8° , for females, 27.2° ; the range of green, for males, 25.5° , for females, 25.6° , or representing the average range of all colors for males at 100%, the average range for females would be 97%. Also in Class I (the seven-year-olds), where all the conditions were equally favorable to both sexes, we find again the males leading by 2%, or representing the average range of the boys for all colors as 100%, the average range for the girls would be represented by 98%. In Classes II and IV we find the conditions less favorable, for in each of these classes there were only two males and four females. Besides, in the class of thirteen-year-olds, where we should expect, if anywhere, to see a difference, the average age of the boys was nearly a year less than that of the girls, and this of itself may have been sufficient to account for the difference in the range in favor of the girls. Representing the average range of the boys as 100%, the average range of the girls would be 103%. In Class IV, where there were two men and four women, the average range favored the men by only 0.1%, or again representing the average range of the men as 100%, the average range of the women would be 99.9%.

These figures, so far as they show anything, show that man possesses not only equal power of recognizing the fundamental colors, but also equal retinal surface sensitive to color.

Let us now turn to the third and last question: What influence has color education on the extent of the range? To the answer of this question the writer brings the results of his observations on twelve different individuals, six of whom were selected from students of the university who had had

no special training in colors, and six others of equal age selected from the students and instructors of the Art Department who had had three or more years of special training in color.

In selecting the first class the author relied on his own judgment, but in selecting the second class he relied principally upon the judgment of Prof. Brown of the Art Department, who himself kindly submitted to the test, and endeavored to recommend only persons who showed special talent in color discrimination and in color harmony. The one class is indicated in the tables as "Adults, Class III," the other, as "Adults trained in color, Class IV."

Perhaps the best and only sure way to answer this question would be to select a number of individuals of the same age and of equal color range, giving to half of them three or four years of special color training, while the others were educated along other lines, without particular reference to color, and then take the color ranges again and compare. But in the absence of data gathered under these conditions, the present carefully made observations are of interest.

Table I shows that with blue the ranges for both the trained and the untrained were practically the same. With yellow, the range was greater for those untrained; while with red there was a decided difference in favor of the trained. The range for green was slightly greater in those untrained. Representing these in percentages and indicating the range of the untrained each time by 100%, we have the following results: The average range for blue in the untrained, 100%, in the trained, 100.2%; the average range for yellow in the untrained, 100%, in the trained, 97%; the average range for red in the untrained, 100%, for the trained, 115%; the average range for green in the untrained, 100%, in the trained, 97%. If the average range for all colors in the untrained were indicated by 100%, the average range for the trained would be 101.8%. This difference, although favoring those trained in color, falls within the individual variations of the same class, and is too small to be taken into account in tests of this kind. And judging both from the figures and the many impressions received during the time the tests were being made, the writer is forced to conclude that color education, as generally understood, has no influence on the color range. But is not this conclusion opposed to the one reached in answer to the first question? If correctly understood it is not. There are two kinds of color education, the direct and the indirect, the conscious and the unconscious, and it is on the indirect and unconscious sort of education, in which all share alike, that the widening of the color field depends.

That these incidental color experiences are necessary to the development of the range is shown by the general form of the color range itself. On the meridians which the nose and eyebrows shield from the color impressions, the retina is sensitive to color over only a few degrees as compared with the rest.

We mean something quite different, however, when we speak of direct color education. When an individual receives color instruction the colored objects are not brought to play on the periphery, but on a small spot at the centre of the retina, and here all color education or training, in the general acceptance of the term, takes place.

There is, therefore, no reason for thinking that those trained in direct color vision ought to see farther in indirect vision than the untrained.

Of other points of interest in this comparison of the trained and untrained may be mentioned the marked difference in the range for red in favor of the trained, of yellow in favor of the untrained, and the greater variety of shades seen by the trained, as well as the greater uncertainty in naming the true color. What color training really does, is to increase the number and variety of the color tones consciously recognized, but it does not increase the amount of retinal surface sensitive to color, and the greater the variety of color tones seen by an individual, the less accurate he becomes in naming the fundamental colors by indirect vision.

Another rather interesting fact was the not unusual occurrence of small color-blind surfaces in eyes otherwise apparently normal. These surfaces varied from 2° to 10° in breadth. Such a color-blind area can be seen in the Tables I to III, by referring under "Class IV" to the outer upper oblique meridian of the right eye of "F." In this case the color-blind area was so situated as to reduce the limit of vision on this meridian several degrees as compared with the other eye. The persistence and irregularity of these spots in some individuals was quite marked.

Again, in the examination of a person who was red-green color-blind, there were found small unequal areas in the eccentric retina of both eyes (near the normal limit for red and green), where all the fundamental colors could be correctly distinguished.

It appears, then (to sum up), 1st, that children cannot see colors as far in indirect vision as adults, but as compared with adults they show a greater proportional range for black and white than for color.

2nd. Difference in sex seems to make no perceptable difference in the extent of the color range.

3d. Color training does not seem to increase the color range (except perhaps in the case of red), but makes itself felt in a greater variety of shades and tints to the colors as seen in indirect vision, and less accuracy in naming the fundamental colors by indirect vision.

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**THIS CHART WILL BE THE FIRST CHART
APPEARING AT THE END OF THIS FILM**

MINOR STUDIES FROM THE PSYCHOLOGICAL
LABORATORY OF CORNELL UNIVERSITY.

COMMUNICATED BY E. B. TITCHENER.

VI.

TASTE DREAMS.

It was long a moot question, to what extent dreams are occasioned by peripheral stimulation, and how large is the part played in them by the centrally excited idea. There seems now to be a practically general agreement in the view that the field of hallucination must be minimized in favor of that of illusion. The influence of external stimuli upon the course of dreaming is probably universal. Thus the *Eigenlicht* of the retina has been called upon to explain the predominance of the visual dream ideas over those of the other senses.¹

Dreams of all kinds, whether peripherally initiated or centrally initiated, imply the presence of a certain trend or disposition of consciousness. The events of the day will have left the mind suggestible in certain principal directions.² It would seem that the "suggestion" must be stronger in the case of dreams which are mainly or exclusively centrally excited, and the central excitation will only be possible where the train of waking thought is simply continued over, after more or less of interruption, into the dream life. The *psychischer Traum* will, therefore, naturally be the rarer type.³ That a sensation following from an external impression shall call up associated ideas along the line of least resistance is more a matter of course; and the determining "suggestion" need not be particularly intensive. Somewhere between these

¹Wundt, *Vorlesungen*, 2d. Ed., p. 352; *Phys. Psych.*, 4th Ed., II, p. 536. James, *Principles*, II, p. 115. Ladd, *Mind*, N. S., I, pp. 299 ff. Spitta, *Die Schlaf- und Traumzustände der menschlichen Seele*, p. 213. Calkins, this JOURNAL, V, p. 319. Etc.

²See Calkins, *l. c.*, pp. 331, 2. Wundt, *Phys. Psych.*, II, p. 540.

³Even so, it is probably seldom found "pure." Cf. Calkins, *l. c.*, pp. 333, 4. I am not referring in the present paper to dreams induced by drugs.

two forms will come the dreams which result from auto-suggestion, from the "will" to dream or not to dream in a particular manner.¹ Here the validity of the volition will be altogether dependent upon circumstances.

Most of our dreaming is in terms of vision. Auditory dreams, especially those in which the auditory ideas are verbal, probably stand high in the order of frequency. Tone-dreams seem to be of rare occurrence. I have certainly dreamed in tonal ideas: *e. g.*, the *Preislied* in the *Meistersinger*. But I have no record, and autosuggestion has failed to induce a musical dream.² Dreams in terms of touch appear to be usually colored by cutaneous pleasure-pain, generally pain.³ Temperature ideas are not uncommon.⁴ Dreaming in terms of the organic sensations is, perhaps, only surpassed in universality by visual dreaming. We have dreams involving the respiratory sensation complex (suffocation, flight, etc.), the static sense (looking or falling from a height, etc.), sex, sensations from stomach and intestines, from the bladder (dreams in which the idea of water plays a part), and from the heart, muscular and movement complexes (resistance, fatigue, etc.), and so on.⁵ Such dreams are subject to a very curious objectification, which usually takes the form of translation into sight or hearing.⁶ Of course, in most instances, the dream ideas of the less frequent senses are found together with the more common visual or auditory ideas. Tones are sung by some person seen, heat is sensed amid certain visual surroundings, etc.—Taste and smell remain.

Wundt remarks that dream hallucinations of taste and smell occur but seldom.⁷ One reason for this is, probably, the difficulty of taste reproduction. "Memorial images of taste impressions are complications, in which the taste sensation proper is of but minimal intensity. It can be altogether replaced by movement sensations, for the reason that these (correlates of movements of mimetic expression) differ for different taste stimuli."⁸ So, too, the memorial representation of smell is composed principally, if not exclusively, of three disparate factors: the visual image of the odoriferous object, the sensation of movement in the nose (inspiration), and the

¹Cf. Nelson, this JOURNAL, I, p. 376.

²Wallaschek, *V. f. Mus. Wiss.*, 1892, pp. 233 ff.; Wundt, *Vorlesungen*, I, c.; Calkins, pp. 319, 322.

³Cf., *e. g.*, the cases cited by Ladd, *Psychology*, 1894, p. 412.

⁴In my own experience. Cf. Calkins, p. 319.

⁵Scherner, *Das Leben des Traumes*, p. 187.

⁶Wundt, *Phys. Psych.*, II, p. 539.

⁷In der Regel fehlen. *Vorlesungen*, p. 358.

⁸*Op. cit.*, p. 310.

touch-temperature complex occasioned by the inspired air. At the same time, this weakness of the true memory image furnishes no valid reason against the cropping up of the vicarious complex idea in the dream series. Taste-smell fusions form a fairly large part of waking conscious content; and the associative suggestiveness of smell impressions is well known.

Miss Calkins¹ found two gustatory presentation dreams in a total of 335 dreams; and four olfactory and no gustatory representation dreams in a total of 298 dreams. In the abstract of Professor Murray's paper, "Do we ever dream of tasting?" in the *Proceedings of the American Psychological Association*² there seems to be a confusion between the classification of dreams as presentative and representative (Calkins) and as illusions and hallucinations. The "representative" dreams include both illusion and hallucination ideas. A dream is no less a dream, because the peripheral sense organ is stimulated during sleep. As we have seen, "whether the central tract . . . can be excited by disturbances in the neighboring tracts without any peripheral stimulation" is a question which may be answered by a theoretical affirmative in the case of all the senses; but it is very doubtful whether, if we had accurate knowledge of the conditions, we should not find illusion to be the dream material in practically every instance; visual, auditory or what not. That is, it does not seem justifiable to single out the taste center as not centrally excitable, *because* it is so very easily excitable peripherally; the same holds of vision:— but there is every reason for supposing that the end organs of taste, like those of vision, are somehow concerned in the suggestion and formation of the dream idea.

During the present year I have collected five good cases of taste dreaming, no one of which is that of a presentation dream (Calkins).

1. On the evenings of January 22, 23 and 24 of the present year, I attempted to induce taste dreams by auto-suggestion. Every precaution was taken to avoid the occurrence of a presentation dream; the mouth thoroughly washed out, etc. There was no indigestion. The first two nights I was unsuccessful; but on the third a perfectly good taste dream occurred. It contained visual, auditory (speech), tactile, muscular (movement of self and others), temperature, affective (both pleasurable and painful), and conative elements, beside the gustatory. The taste dreamed of was that of

¹L. c., pp. 319, 321.

²New York, 1894, pp. 20, 21.

English school plum cake. All the elements of reproduction were present; the visual and motor idea of breaking off fragments from a slice, the tactile sensations from their crumbling in the mouth, taste and aroma. The dream continued beyond the taste part of it. Jotted down immediately after waking, the dream record comprised 300 to 350 words.¹ The dream was of the morning class.² It was, however, a true sleeping dream.³ I had arranged to be awaked somewhat earlier than usual, in order to prevent the confusion of dozing with sleep proper, and was on the morning of January 25 aroused from sound sleep. On waking, I had the normal saliva taste in the mouth, which appeared on re-testing to be perfectly free from food fragments. The associative connections between the dream and events of the waking life were traceable with rather exceptional completeness. This may have been due, in part at least, to the fact of autosuggestion.

2. The second instance appears also to belong to the class of suggested dreams. It was recorded by the Rev. A. Beede, Alfred, Me., who had seen a notice of Professor Murray's paper in the *Philosophical Review*, and "resolved to watch for an opportunity of verifying" the occurrence of taste dreaming in his own experience. The dream took place in the night of Feb. 15, 1894. The mouth was clean. There was, perhaps, a very slight indigestion. The dream contained visual, tactile, muscular (movement of self), affective (both pleasurable and painful) and conative elements, beside the gustatory. The taste dreamed of was that of fresh strawberries; two good, one over-ripe. Reproductive elements present were: picking of the fruit and placing it in the mouth, taste (pleasant and unpleasant) and aroma, two acts of swallowing and (?) one of spitting out. The dream continued after the taste part of it. The letter of communication contains 350 to 400 words. The dream was of the night class. On waking, the taste in the mouth was, perhaps, not quite normal; this, like the presence of slight indigestion, is doubtful. The associative connections between the dream and events of the waking life were traceable with very considerable completeness.

It does not seem necessary to transcribe the remaining three dreams in detail. Each dream is reported by a different observer; and I have every reason to believe the reports trustworthy. No one of the three was autosuggested.

¹Nelson, p. 399.

²Calkins, p. 318.

³Nelson, p. 353.

These five are all "representative" dreams; and with the possible exception of two, as "hallucinatory" as dreams can well be. If the evidence be still regarded as unconvincing, I would propose that trial be made of autosuggestion. This can, of course, be done without any lapse into that form of the "psychologist's fallacy," against which Professor Murray cautions dream observers.¹

Postscript. Since the above paper was sent in to the Editor, Sept. 14, I have received accounts of three more taste dreams; two from new observers. No one of them was suggested. Since that date, also, there has appeared Professor Ribot's article, *Recherches sur la Mémoire affective* (*Rev. phil.*, Oct., 1894), which confirms many of my arguments.

VII.

ON THE QUANTITATIVE DETERMINATION OF AN OPTICAL ILLUSION.

(Continued.)

BY R. WATANABE, PH. D.

On page 418 of the current volume of this JOURNAL, Mr. Knox writes, apropos of the dotted-line and point-distance illusion, as follows: "Binocular bisection of horizontal distances is not subject to any constant error; binocular bisection of verticals is subject to the constant error of over-estimation of the upper part of the field of vision. We should, therefore, expect to find the *m. v.* of our vertical Δ 's greater than that of our horizontal. The results [do not verify this expectation] This is curious. We are unable to offer any explanation of the result."

Further experiments upon the illusion in question were made, in the hope of elucidating this difficulty. Every precaution that could be thought of was taken to ensure accuracy and avoid the intrusion of complicating factors. Mr. Knox' experiments were exactly repeated, with a single modification. Whereas, on his cards, the point-distance was constant, and the dotted-line variable, on our own the reverse was the case. We imagined that if this alteration in the nature of stimulus brought about any alteration in judgment, the latter would be of such a kind as to be readily determinable for itself; and that this determination, itself an interesting side issue, would not interfere with the realization of the main object of the new experiments. On the other hand, the stimulus altera-

¹P. 21.

tion might prove to be without influence upon the judgment process; in which case we should at least obtain the negative result.

Series were obtained from three subjects: Messrs. Knox (*K.*; see pp. 416, 419), Pillsbury (*P.*; see pp. 417, 419, 421), and Read (*R.*). The following tables correspond in every respect to those of pp. 415 ff.:

TABLE I.

Reagent *K.* Vision normal. Method (*e*) predominant. General and special practice. Unit = 1 mm.

SERIES.	<i>R.</i>								
	<i>C</i> =25	<i>n.</i>	<i>m.v.</i>	<i>C</i> =30	<i>n.</i>	<i>m.v.</i>	<i>C</i> =35	<i>n.</i>	<i>m.v.</i>
<i>C-V</i>	27.18	3	0.62	32.12	3	0.62	36.62	3	0.78
<i>V-C</i>	25.25	3	1.66	32.00	3	1.25	36.95	3	0.80
$\frac{V}{C}$	26.29	3	0.82	31.95	3	0.63	37.16	3	0.63
$\frac{V}{C}$	26.71	3	0.90	31.37	3	0.73	36.95	3	0.64
<i>Hor. Δ</i>	+1.21			+2.12			+1.78		+2.31
<i>Vert. Δ</i>	+1.50			+1.66			+2.05		+1.95

TABLE II.

Reagent *P.* Vision Normal. Method (*b*) predominant. General and special practice. Unit = 1 mm.

SERIES.	<i>R.</i>								
	<i>C</i> =25	<i>n.</i>	<i>m.v.</i>	<i>C</i> =30	<i>n.</i>	<i>m.v.</i>	<i>C</i> =35	<i>n.</i>	<i>m.v.</i>
<i>C-V</i>	25.08	12	0.81	30.89	11	0.56	35.78	12	0.66
<i>V-C</i>	25.40	12	0.69	31.65	12	0.58	36.58	12	0.80
$\frac{V}{C}$	26.48	9	0.57	32.84	9	0.65	37.81	9	0.72
$\frac{C}{V}$	26.44	9	0.86	32.40	9	0.68	37.37	9	0.85
<i>Hor. Δ</i>	+0.24			+1.27			+1.18		+1.31
<i>Vert. Δ</i>	+1.46			+2.62			+2.59		+3.10

TABLE III.

Reagent *R.* Vision normal. Special practice only. Method mixed.
Unit = 1 mm.

SERIES.	<i>R.</i>											
	<i>C</i> =25	<i>n.</i>	<i>m.v.</i>	<i>C</i> =30	<i>n.</i>	<i>m.v.</i>	<i>C</i> =35	<i>n.</i>	<i>m.v.</i>	<i>C</i> =40	<i>n.</i>	<i>m.v.</i>
<i>C-V</i>	27.10	6	1.27	31.39	6	1.60	37.26	6	1.39	41.89	6	1.17
<i>V-C</i>	27.49	6	1.08	32.83	6	1.32	37.53	6	1.48	42.49	6	1.41
$\frac{V}{C}$	27.37	6	1.27	32.29	6	1.51	37.66	6	1.57	42.41	6	1.47
$\frac{C}{V}$	26.72	6	1.37	31.25	6	1.33	36.83	6	1.22	41.74	6	1.33
<i>Hor. Δ</i>	+2.29			+2.11			+2.39			+2.19		
<i>Vert. Δ</i>	+2.04			+1.77			+2.24			+2.07		

*Remarks.*¹ (1) The illusion holds for every observer.

(2) Vertical Δ 's are larger than horizontal, in these twelve comparisons, in six cases; smaller in six. But in none of these contrary cases does the difference of the two Δ 's amount to half a mm. (Differences are: Table I—0.46, 0.36; Table III—0.25, 0.34, 0.15, 0.12.) Moreover, four of them come from the least practised reagent, *R.*, who began with horizontal judgments. Mr. Knox' conclusion under this head is, therefore, confirmed by our results.

(3) The main object of the present investigation has been stated above. Do we find any light thrown upon the matter by the present figures? We have:

Table I. $r\ 0, = 2$ (.79, .63; .97, .93), $w\ 2$ (1.72, 0.86; .93, .68).

Table II. $r\ 0, = 4$ (.75, .71; .57, .66; .73, .78; .92, .87), $w\ 0$.

Table III. $r\ 2$ (1.17, 1.32; 1.29, 1.40), $= 2$ (1.46, 1.42; 1.43, 1.39), $w\ 0$.

In all, $r\ 2, w\ 2, = 8$. Mr. Knox obtained $r\ 6, w\ 7, = 7$; or, if his Table I be included, $r\ 8, w\ 8, = 8$. Massing, therefore, we get $r\ 10, w\ 10, = 16$. We do not insist upon the absolute relations of these figures,—apart from the fact that massing, even in two such comparable cases as these, is psychologically unjustifiable. Nor do we fail to note that of

¹See pp. 418 ff.

our own = 8, six have what tendency to differ they do have in the direction of w ; of Mr. Knox' = 8, seven have such a tendency. But we feel safe in formulating the following proposition: *so far as Mr. Knox' and our own experiments extend, there is strong evidence that, in presence of the dotted-line and point-distance illusion, the illusion of over-estimation of the upper half of the field of vision disappears*; the evidence being couched in terms of the $m. v.$ in vertical and horizontal quantitative determinations of the former illusion. We are not at present prepared to suggest any explanation of this fact. The fact itself holds, whether we employ the method with knowledge or the method without knowledge, and whether the reagent be practised or comparatively unpractised in *Augenmass* experiments.

(4) The values of $\frac{\Delta}{r}$ are:

I. Hor.: $\frac{1}{20}, \frac{1}{14}, \frac{1}{19-20}, \frac{1}{17}$. Vert.: $\frac{1}{17}, \frac{1}{18}, \frac{1}{17}, \frac{1}{20-21}$.

II. Hor.: $\frac{1}{104}, \frac{1}{23-24}, \frac{1}{29-30}, \frac{1}{30-31}$. Vert.: $\frac{1}{17}, \frac{1}{11-12}, \frac{1}{13-14}, \frac{1}{13}$.

III. Hor.: $\frac{1}{11}, \frac{1}{14}, \frac{1}{14-15}, \frac{1}{19-20}$. Vert.: $\frac{1}{12}, \frac{1}{16-17}, \frac{1}{15-16}, \frac{1}{19-20}$.

(a) Table I is taken from the same reagent as Mr. Knox' Table III. The values of its limina are probably vitiated for the reason alleged on page 419. (b) Table II is taken from the over-practised reagent *P*. (See page 419.) Here, as in Mr. Knox' Table IV, the vertical Δ 's have suffered much less by practice than have the horizontal. (c) Table III, from a previously unpractised reagent, confirms Mr. Knox' general formula (p. 419) a good deal better than his own Table I, from a similar reagent, does. (d) The valuelessness of $C = 40$ mm. is indicated by Table III. We should not expect to find evidence of it in the other two tables. It is, perhaps, hardly necessary to make the explicit statement that these supplementary experiments were not at all expected to throw light on the magnitude of $\frac{\Delta}{r}$.

The two reagents *K*. and *P*. were wholly unsuitable for such a purpose. On the other hand, the reasons that disqualify them for that investigation do not come into account for the main issue, discussed under (3). And it is, at least, satisfactory to note that there is nothing in the fractions which makes against Mr. Knox' conclusions. Thus, Tables I and II alike make the vertical $\frac{\Delta}{r}$ greater, on the average, than the horizontal; and the variation from this rule, in Table III, is so slight as

to be readily explicable in terms of the order of special practice (*cf.* Table I, p. 419). (*e*) Did the reversal of the *C* and *V*, as compared with those of Mr. Knox' investigation, influence the process of judgment? (i) The opinion of the reagents *K.* and *P.* was to the effect that it did not. (ii) If we compare the fractions obtained by the old and new methods, we find that those of the latter case are:

Table I. *Hor.*: $\langle \langle \langle \langle \rangle \rangle \rangle$; *Vert.*: $\langle \langle \langle \rangle \rangle \langle$.

Table II. *Hor.*: much $\langle = \langle \langle$; *Vert.*: \langle just $\langle = =$, as compared with those of the former. This general lessening was to have been expected, other things equal, from the increase of practice. Had the interchange of *C* and *V* had any influence, it would, we think, have been one in opposition to this tendency to lessen. For *a priori*, if there is any question of relative ease or difficulty, it should be easier to estimate when the dotted line varies (Mr. Knox' procedure) than when the point distance is the variable (our own). In the former case, an extension difference carries with it a quality difference, a greater or less number of dots: while, when the extension of the point-distance alters, no qualitative change is involved. The fact that the practice-lessening of the fractions is so little counteracted, therefore, in our results, tends to confirm the verdict of introspection. (iii) Moreover, the horizontal figures of Table III show, as has been pointed out, a very good agreement with Mr. Knox' formula. On the whole, then, we would answer the question of this paragraph in the negative. (*f*) Since the appearance of Mr. Knox' paper, there has been published in the *Zeitschr. f. Psych. u. Physiol. d. Sinnesorg.*, an attempt at a quantitative treatment of another optical illusion—that of the arrow head and feather (*F. Auerbach: Erklärung der Brentanoschen optischen Täuschung*, Vol. VII, pp. 152 ff.). The numerical results (p. 159) are not comparable with those given in the two present papers; the point investigated being not the quantitative variation of the illusion with variation of absolute magnitude of lines or distances, but its increase with increasing length of the limbs of the limiting right angles. But attention may be called to certain remarks of the writer's, bearing upon the general question. (i) The illusion varies with the *visual habits* of the reagent (p. 155). We have had no opportunity of testing this, in Auerbach's way. But the statement receives indirect confirmation from Mr. Knox' conclusions, p. 419 of this JOURNAL. (ii) Increased concentration of vision and attention diminishes the illusion (p. 155). This holds of the arrow head and feather illusion, for the explanation of which the influence of indirect vision is called into account, to a greater extent than for our own.

At the same time we have seen that a similar result may be obtained by familiarity and practice. (iii) It is necessary, for determination of the limen of difference, to avoid knowledge of the actual relations of the distances compared on the part of the reagent (p. 159). This point has also been insisted on by Mr. Knox. (iv) Judgment should be as immediate as possible; since it is apt to fluctuate, if the stimulus is present for any length of time (p. 159). Cf. the length of Mr. Knox' experimental series (p. 414). The method employed both by Auerbach and ourselves being a form of minimal changes, the necessity of immediacy of judgment is a matter of course.

VIII.

THE CUTANEOUS ESTIMATION OF OPEN AND FILLED SPACE.

BY PROFESSOR C. S. PARRISH.

A comparison of the spatial functioning of the cutaneous and visual sensibilities must always possess an especial psychophysical interest. The study of sensational intensities culminates in Weber's Law; that of sensational quality leads to a whole number of alternative psychophysical theories; the determination of the temporal attributes of sensation is one means of approaching the problems of the so-called time-sense; that of its spatial attributes, the first step towards a psychological space construction. But, whereas every sensation is possessed of duration, quality, and (with the exception of the visual series) intensity, a space attribute attaches exclusively to the sensations of sight and pressure. This fact, which seems at first sight to simplify the space problem, in reality renders that problem unusually difficult of solution.

Opinions differ very widely as regards the sensational factor in psychological space, as regards the interaction of eye and skin in its construction, and as regards the attributes and aspects of the cutaneous sensibility itself. It may, therefore, be well to give here, at the outset, a brief *credo*, not with any intention of dogmatizing, but merely with a view to clearness and intelligibility.

We believe, then, that the development of the eye, as a space organ, far outran that of the skin. That tactual space was, accordingly, built up under the influence of, and remains almost invariably subject to that of vision. Nevertheless, that there are two psychological spaces, and not one space. We consider, further, that the mechanical cutaneous sensi-

bility has (*pace* Dessoir) only one quality, that of pressure. That the cutaneous local signature is physiological only, although the retinal—as is indicated by many facts which cannot here be adduced—is psychological. And that in its ordinary functioning, the skin co-operates with the three deeper lying sensibilities, the tendinous (with its quality of strain), the articular (with its quality of pressure, and possibly with its own local signature), and the muscular (with its peculiar quality which only becomes seriously involved in fatigue or exhaustion). Finally, we ascribe to the cutaneous and visual sensation an attribute of extension, which we regard as co-ordinate with intensity, quality and duration, and which is by no means to be confused with the “bigness” or “massiveness” predicate of one school of nativistic psychologists.

After this preface we may approach the special problem which heads this paper. Almost unexceptionally, the eye regards a filled space as greater than an empty space objectively equal to it. What is the attitude of the skin to such spaces? It has recently been maintained that pressure *plus* movement functions, in this regard, as does the eye.¹ But one of the explanations propounded for the visual illusion is couched in terms of movement: it being argued that though the resting eye is also subject to the error, it is only so subject because, at some time or another, it has moved. The argument is paralleled by many others of the chapter of psychology which deals with visual perception, and need not be further commented on. Now, if the resting skin (*sit venia verbo!*) were deluded equally with the moving, then, although visualization might be called in to explain the fact, we should still be in presence of a phenomenon telling with more or less of force against the movement theory. If, however, the resting skin, in spite of visualization, and in spite of its own constant movement in the past, should prove to be not deluded, the movement theory is so far supported. Should the illusion be actually reversed, we must look for the conditions of such reversal in the special psychophysics of the organ.

The problem, then, resolves itself into that of obtaining comparative space judgments from the resting skin. If the skin, and the skin only, is to be appealed to, stimulation must be liminal. In this case, however, judgment will be uncertain, and comparison difficult. Since in ordinary life the static functioning of the skin is almost invariably correlated with a similar functioning of the deeper lying sensibilities—since, *i. e.*, normal stimulation is almost always supra-

¹Dresslar, this JOURNAL, pp. 332 ff. The view of this author is criticised below.

Method.—We imagined, at the outset of the investigation, that a very large number of experiments would be needed, if any satisfactory result was to be obtained. We therefore employed a modification of the method of right and wrong cases. Instead of taking too little-different stimuli, as that method requires, we proposed to compare each block with every other block, recording the judgments as r , w , = and ? [r being used on the optical analogy, when the cutaneous judgment made a filled larger than an objectively equal empty space]; while, to avoid *Einstellung*, we did not confine ourselves to one pair of blocks in each series of experiments, but intermixed the comparisons at random. But the uniformity of which we were in search made its appearance so quickly, decidedly and unmistakably, that this original plan was not carried out. We have, consequently, only a relatively small number of experiments taken by this method, and those we propose to submit in detail.

In the following Table the first column gives the blocks compared, the numbers (as stated above) signifying the number of points in each block; while in the others, each of which is accredited to a different reagent, the signs ($>$ or $<$) gives the judgment of relation recorded N times out of n experiments. Thus, " $2:3 > 4:6$ " means that the two-point distance was judged greater than the three-point distance four times out of six experiments, by the particular reagent. Of the seven subjects, one only, (T%) had had general as well as special practice. His results, although not numerous, are the most reliable. The other six reagents were specially practised for the purposes of this investigation. It should be stated that during the first half (approximately) of these experiments, the application of the second stimulus was not to the exact part of the skin stimulated by the first, but to a line just alongside of it; during the second half the successive applications were made at precisely the same place. Absolutely no difference in result could be discovered; and irradiation makes this intelligible. Both sets of experiments have, therefore, been drawn upon in the composition of the Table.

Remarks.—(1) We notice at once that, for the resting skin, a filled distance is, on the average, shorter than an empty distance objectively equal to it. This holds for every reagent. In the earlier stages of practice, when visualization was especially insistent, there occurred sporadic cases to the contrary effect: the filled distance appeared longer. But such cases disappeared as practice progressed; and introspection referred them very definitely to the influence of the visual idea.

TABLE I.

	B.	Ha.	H.	O.	Ol.	T.	Tv.
	<i>N</i> <i>n</i>	<i>N</i> <i>n</i>	<i>N</i> <i>n</i>	<i>N</i> <i>n</i>	<i>N</i> <i>n</i>	<i>N</i> <i>n</i>	<i>N</i> <i>n</i>
2 : 3	> 4 6	> 2 2	> 2 4	> 5 6	> 4 5	> 7 11	> 4 4
: 4a	" 5 6	< 4 5	" 7 13	" 1 2	" 5 6	" 5 8	" 2 2
: 4b	" 3 4	—	" 3 4	" 5 5	" 4 5	" 3 3	—
: 5	" 4 6	> 5 6	" 4 6	" 5 7	" 3 5	" 5 7	" 5 6
: 6	" 3 4	" 3 3	" 4 6	" 4 4	" 3 3	" 4 6	" 2 2
: 7	" 2 4	" 3 4	" 3 4	" 4 4	" 2 3	" 9 10	" 2 2
: 8	" 4 6	" 1 2	" 5 5	" 6 7	" 5 5	" 3 4	—
: 9	" 2 4	" 2 2	" 4 4	" 5 6	" 4 4	" 4 4	—
3 : 4a	" 3 4	< 4 5	< 3 5	" 2 4	" 5 6	< 4 4	—
: 5	" 6 9	> 4 6	> 3 4	" 6 8	" 4 4	> 5 8	" 4 7
: 6	—	" 3 4	" 3 6	" 6 8	" 3 4	" 5 6	—
: 7	" 1 2	" 7 8	" 4 8	" 5 5	" 5 7	" 8 11	" 4 4
: 8	" 1 2	" 3 5	" 3 6	" 2 4	" 3 4	" 4 5	" 2 2
: 9	" 1 1	" 3 6	—	" 2 3	" 2 2	" 6 8	" 3 4
4a : 5	" 4 7	" 2 3	" 2 4	" 3 3	" 2 4	" 1 2	—
: 6	" 1 2	" 1 2	< 4 6	" 6 8	" 2 3	" 3 4	—
: 7	" 5 6	" 4 6	> 5 7	" 3 3	" 2 2	" 4 8	—
: 8	< 3 4	" 1 2	" 5 8	" 4 4	" 3 4	" 3 4	" 2 2
: 9	> 2 3	" 1 2	—	" 2 3	" 2 3	" 2 4	—
4b : 5	—	< 2 2	—	" 2 3	" 2 3	" 2 4	—
: 6	—	—	—	" 3 4	" 2 4	" 2 3	—
: 7	—	—	" 2 3	" 2 3	" 2 3	" 4 4	—
: 8	" 1 2	—	< 1 2	" 2 3	> 2 3	" 2 3	—
: 9	—	—	" 2 3	" 2 3	> 2 3	" 2 4	—
5 : 6	" 2 3	" 2 2	" 3 4	" 2 3	—	" 2 2	—
: 7	" 5 8	> 4 5	" 2 3	" 5 6	" 3 4	" 7 7	" 3 4
: 8	" 1 2	< 3 4	" 3 5	< 5 6	< 1 2	" 2 3	" 2 2
: 9	" 2 4	> 2 4	" 1 2	" 3 3	> 2 3	" 6 8	" 5 6
6 : 7	" 1 2	" 1 2	" 2 2	> 2 3	" 2 3	" 3 4	" 2 2
: 8	—	—	" 2 2	" 3 4	" 2 3	" 4 4	—
: 9	—	—	" 3 4	" 2 3	" 2 3	" 3 6	—
7 : 8	—	" 2 4	—	" 2 3	" 2 3	" 4 6	—
: 9	—	< 2 2	—	" 3 6	< 2 3	" 4 7	—
8 : 9	—	—	—	" 4 8	> 2 4	" 4 5	—

(2) There are a few exceptions to this rule. To understand them we must ask at once—taking the confirmatory evidence of Series II for granted—for the reason of the reversal of the illusion, in its transference from optics to haptics. That reason is given in terms of introspection by the reagents. All alike asserted that the points in the filled line were sensed as "bunched" or "crowded" together. The space between two points can be fairly accurately apprehended—we are not speaking, of course, of objective accuracy—but a space which is more or less filled "shrinks" together, and may be reduced to what are comparatively very small proportions. Doubtless, for the majority of the subjects, the first judgment was more influenced by visualization than was the second. But this is

an error common to almost all cutaneous experiments. And, though it may have aided the reversal of the illusion, it certainly could not have produced it. Moreover, the reagent *Ti.* has been able, by practice, pretty completely to separate the visual from the cutaneous judgment,¹ and the reversed illusion holds quite strongly in his case. The "bunching" or "crowding" is psychophysically explicable in terms of irradiation.

(3) Now for the exceptions. (a) $2 < 4a$ for *Ha.* Reference to the scheme will show that $4a$ may be regarded as an open line, doubly bounded at either extremity. This double bounding would make the point-impressions especially intensive. Now it proved to be a constant error in these experiments—one which evidenced itself in the practice series, in which experimenter was being educated as well as experimentee—that *increased intensity of pressure meant a judgment of increased length of line.* We did not attempt any quantitative evaluation of this error; the error itself was eliminated by the acquisition of facility and accuracy in the handling of the blocks. But we suggest that the error, in a modified form, may account for these exceptional judgments. (b) $3 < 4a$ for *Ha.*, *H.* and *T.* This is, again, easily accounted for. 3 is a filled line; $4a$ may be sensed as a doubly bounded open line. When this is the case, the judgment $<$ will follow, in terms of the cutaneous illusion. (c) $4a < 6$ for *Ha.* This we can only explain by supposing that the dots in the middle of 6 were crowded together in sensation, the terminal points being thus left free. (d) $4a < 8$ for *B.* Again, we have possibly a similar explanation. The two 4's of 8 are crowded, leaving the centre space free; $4a$ is more irregularly filled for sensation. Introspection gave a confirmatory result in both these cases. (e) $4b < 5$ for *Ha.* The two blocks are so similar, that any accidental and variable factor may have conditioned this judgment. (f) $4b < 7$ for *O.* and *Ol.* Here, again, the blocks are of like patterns. Perhaps the central dots of 7 were massed, leaving the ends free. (g) $4b < 8$ for *H.* and *T.*; cf. (d) above. (h) $5 < 6$ for *Ha.* The former is more uniformly filled. (i) $5 < 8$ for *Ha.*, *O.* and *Ol.*; cf. (d) and (g) above. (k) $5 < 9$ for *O.* This cannot be explained by reference to the blocks. (l) $7 < 9$ for *Ha.* and *Ol.* Nor can this.

We would call attention to the facts: (i) that these are very few exceptions, *when the fewness of the experiments in*

¹Proof of this statement will be advanced later, in articles by Dr. M. F. Washburn and Mr. W. B. Pillsbury, dealing respectively with the influence of the visual idea upon cutaneous space judgments, and upon cutaneous localization.

could recognize color only three-fourths as far. Again, the seven-year-olds could see objects 78% as far as adults, while they could see color only 61% as far. Although it is difficult to decide what the true cause is, yet the writer believes that the cause must be looked for in the order of the color development itself, whatever may be regarded as the true theory of color vision. Beginning with the visual color field, as seen by the adult, and passing backward toward and through the visual field of the child, we notice that the field not only decreases in extent but also becomes more circular. The same is true of the general visual field, *i. e.*, the visual field for light and shade, only the latter field decreases much less rapidly, or, in other words, shows a much greater proportional range in children than the color field. Now, should we continue still farther backward, and this law hold true, we should finally reach a point in the child's development where the eye ceased to be sensitive to color impressions; and from previous observations on young children, the writer would place this point not earlier than the fourth week of life, if as early as that. Perhaps no one will doubt the statement that the child becomes sensitive to light and shade much sooner than to color as color. A very young child may be attracted by a bright color, not on account of the color but on account of the light it contains. The above observations show that light and shade either develop faster in children than color, or else start sooner to develop, for while the range for light and shade of the seven-year-olds, as compared with adults, stands seventy-eight to 100, color, for the same individuals, stands as sixty-one to 100, linear measure. The writer very much questions whether his own children were able to perceive blue before they were eighteen months old, while they knew and seemed to enjoy red, orange and yellow very much earlier. If it be true that blue is the last of the colors to be perceived by the child, it becomes interesting to know why in after life blue has the greatest range of all the colors.

Judging from all the evidence at hand it seems probable that the child inherits from past generations an ever increasing color tendency, but nothing more, that he must come in contact with the real colors, or, in other words, the different color stimuli must play on the retina in order to develop this color tendency into a real mechanism for the discrimination of color, and that this mechanism begins to develop in earliest childhood, develops slowly and is finally completed in adult life. Further discussion of this point must be reserved for a future paper.

The second question was: Does sex have anything to do with difference in the range? There is a prevalent belief that woman has a better color eye than man, *i. e.*, greater inherent power of seeing and discriminating color, and that there are more color-blind men than women. The writer doubts, however, whether these beliefs are founded on facts. At least a carefully arranged and conducted experiment on 200 children (a number too small, perhaps, for accurate judgment) showed practically no sex differences in color vision. In the present experiment on visual range the data are entirely too limited to warrant any definite conclusions in the matter of sex, but the figures at least do not show that woman possesses any color superiority over man. In Tables I to IV, we find in Class III, designated adults, where there were three male and three female observers, that the average range for the males with blue was 47.4° , for the females, 45.7° ; the range of yellow, for males, 46.3° , for females 44° ; the range of red, for males, 27.8° , for females, 27.2° ; the range of green, for males, 25.5° , for females, 25.6° , or representing the average range of all colors for males at 100%, the average range for females would be 97%. Also in Class I (the seven-year-olds), where all the conditions were equally favorable to both sexes, we find again the males leading by 2%, or representing the average range of the boys for all colors as 100%, the average range for the girls would be represented by 98%. In Classes II and IV we find the conditions less favorable, for in each of these classes there were only two males and four females. Besides, in the class of thirteen-year-olds, where we should expect, if anywhere, to see a difference, the average age of the boys was nearly a year less than that of the girls, and this of itself may have been sufficient to account for the difference in the range in favor of the girls. Representing the average range of the boys as 100%, the average range of the girls would be 103%. In Class IV, where there were two men and four women, the average range favored the men by only 0.1%, or again representing the average range of the men as 100%, the average range of the women would be 99.9%.

These figures, so far as they show anything, show that man possesses not only equal power of recognizing the fundamental colors, but also equal retinal surface sensitive to color.

Let us now turn to the third and last question: What influence has color education on the extent of the range? To the answer of this question the writer brings the results of his observations on twelve different individuals, six of whom were selected from students of the university who had had

no special training in colors, and six others of equal age selected from the students and instructors of the Art Department who had had three or more years of special training in color.

In selecting the first class the author relied on his own judgment, but in selecting the second class he relied principally upon the judgment of Prof. Brown of the Art Department, who himself kindly submitted to the test, and endeavored to recommend only persons who showed special talent in color discrimination and in color harmony. The one class is indicated in the tables as "Adults, Class III," the other, as "Adults trained in color, Class IV."

Perhaps the best and only sure way to answer this question would be to select a number of individuals of the same age and of equal color range, giving to half of them three or four years of special color training, while the others were educated along other lines, without particular reference to color, and then take the color ranges again and compare. But in the absence of data gathered under these conditions, the present carefully made observations are of interest.

Table I shows that with blue the ranges for both the trained and the untrained were practically the same. With yellow, the range was greater for those untrained; while with red there was a decided difference in favor of the trained. The range for green was slightly greater in those untrained. Representing these in percentages and indicating the range of the untrained each time by 100%, we have the following results: The average range for blue in the untrained, 100%, in the trained, 100.2%; the average range for yellow in the untrained, 100%, in the trained, 97%; the average range for red in the untrained, 100%, for the trained, 115%; the average range for green in the untrained, 100%, in the trained, 97%. If the average range for all colors in the untrained were indicated by 100%, the average range for the trained would be 101.8%. This difference, although favoring those trained in color, falls within the individual variations of the same class, and is too small to be taken into account in tests of this kind. And judging both from the figures and the many impressions received during the time the tests were being made, the writer is forced to conclude that color education, as generally understood, has no influence on the color range. But is not this conclusion opposed to the one reached in answer to the first question? If correctly understood it is not. There are two kinds of color education, the direct and the indirect, the conscious and the unconscious, and it is on the indirect and unconscious sort of education, in which all share alike, that the widening of the color field depends.

That these incidental color experiences are necessary to the development of the range is shown by the general form of the color range itself. On the meridians which the nose and eyebrows shield from the color impressions, the retina is sensitive to color over only a few degrees as compared with the rest.

We mean something quite different, however, when we speak of direct color education. When an individual receives color instruction the colored objects are not brought to play on the periphery, but on a small spot at the centre of the retina, and here all color education or training, in the general acceptance of the term, takes place.

There is, therefore, no reason for thinking that those trained in direct color vision ought to see farther in indirect vision than the untrained.

Of other points of interest in this comparison of the trained and untrained may be mentioned the marked difference in the range for red in favor of the trained, of yellow in favor of the untrained, and the greater variety of shades seen by the trained, as well as the greater uncertainty in naming the true color. What color training really does, is to increase the number and variety of the color tones consciously recognized, but it does not increase the amount of retinal surface sensitive to color, and the greater the variety of color tones seen by an individual, the less accurate he becomes in naming the fundamental colors by indirect vision.

Another rather interesting fact was the not unusual occurrence of small color-blind surfaces in eyes otherwise apparently normal. These surfaces varied from 2° to 10° in breadth. Such a color-blind area can be seen in the Tables I to III, by referring under "Class IV" to the outer upper oblique meridian of the right eye of "F." In this case the color-blind area was so situated as to reduce the limit of vision on this meridian several degrees as compared with the other eye. The persistence and irregularity of these spots in some individuals was quite marked.

Again, in the examination of a person who was red-green color-blind, there were found small unequal areas in the eccentric retina of both eyes (near the normal limit for red and green), where all the fundamental colors could be correctly distinguished.

It appears, then (to sum up), 1st, that children cannot see colors as far in indirect vision as adults, but as compared with adults they show a greater proportional range for black and white than for color.

2nd. Difference in sex seems to make no perceptable difference in the extent of the color range.

3d. Color training does not seem to increase the color range (except perhaps in the case of red), but makes itself felt in a greater variety of shades and tints to the colors as seen in indirect vision, and less accuracy in naming the fundamental colors by indirect vision.

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CHART 1.

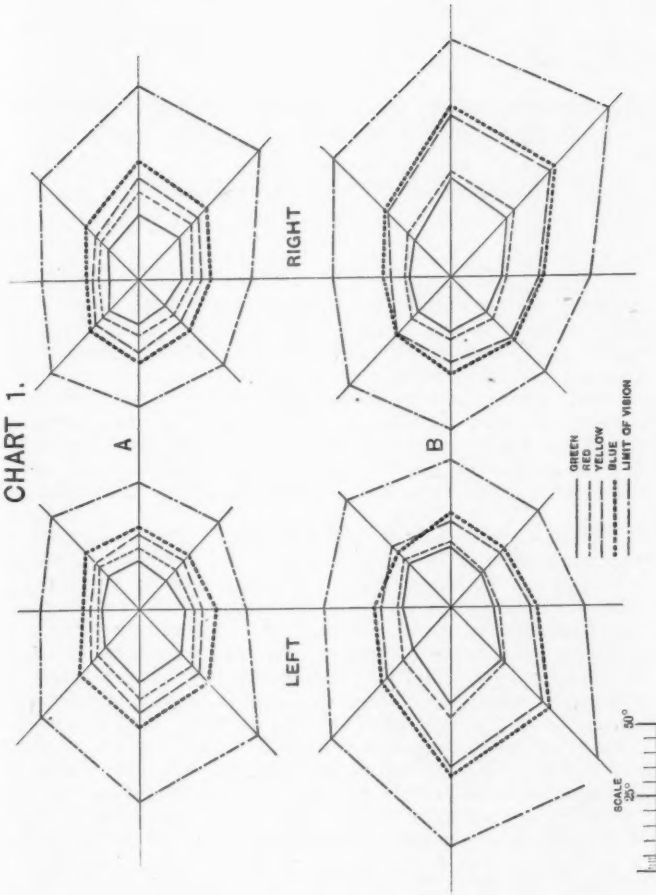
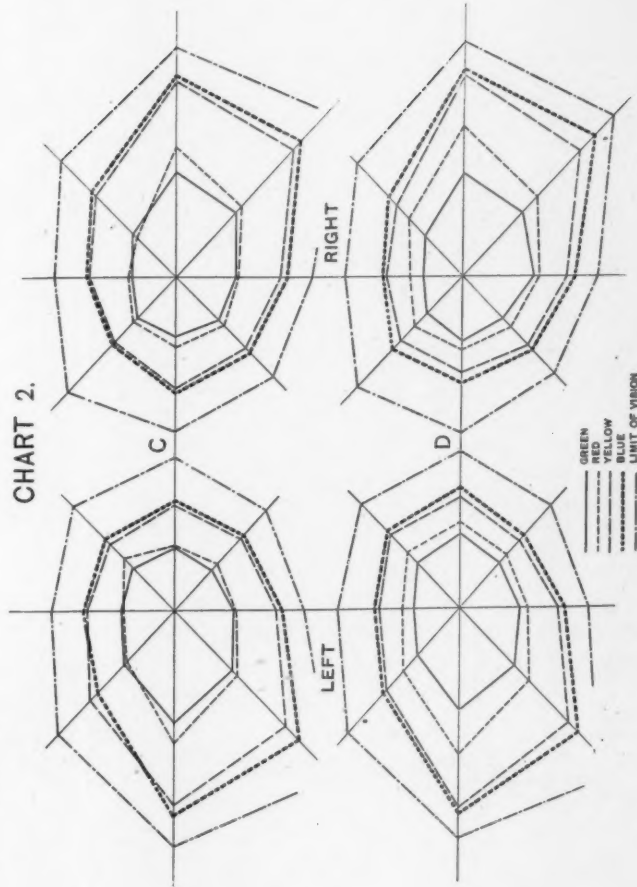


CHART 2.



EXPLANATION OF CHARTS 1 AND 2.

CHART 1. A represents the average projected color field of the left and right eyes of six children seven years old. B represents the average projected color field of six children thirteen years old. C represents the average projected color field of six adults. D represents the same field of six adults who had had three or more years' special training in color. The line marked "limit of vision," is the average distance at which the class could see a white object of the same size as the colored objects used. By an unfortunate error in the reproduction of the original drawings Chart 2 is not shown on the same scale as Chart 1, but on a smaller scale. For this reason the limits of vision in C and D seem smaller than in B, though in reality they are clearly larger. The straight radiating lines are the meridians along which the colors were brought into the field of vision.

MINOR STUDIES FROM THE PSYCHOLOGICAL
LABORATORY OF CORNELL UNIVERSITY.

COMMUNICATED BY E. B. TITCHENER.

VI.

TASTE DREAMS.

It was long a moot question, to what extent dreams are occasioned by peripheral stimulation, and how large is the part played in them by the centrally excited idea. There seems now to be a practically general agreement in the view that the field of hallucination must be minimized in favor of that of illusion. The influence of external stimuli upon the course of dreaming is probably universal. Thus the *Eigenlicht* of the retina has been called upon to explain the predominance of the visual dream ideas over those of the other senses.¹

Dreams of all kinds, whether peripherally initiated or centrally initiated, imply the presence of a certain trend or disposition of consciousness. The events of the day will have left the mind suggestible in certain principal directions.² It would seem that the "suggestion" must be stronger in the case of dreams which are mainly or exclusively centrally excited, and the central excitation will only be possible where the train of waking thought is simply continued over, after more or less of interruption, into the dream life. The *psychischer Traum* will, therefore, naturally be the rarer type.³ That a sensation following from an external impression shall call up associated ideas along the line of least resistance is more a matter of course; and the determining "suggestion" need not be particularly intensive. Somewhere between these

¹Wundt, *Vorlesungen*, 2d. Ed., p. 352; *Phys. Psych.*, 4th Ed., II, p. 536. James, *Principles*, II, p. 115. Ladd, *Mind*, N. S., I, pp. 299 ff. Spitta, *Die Schlaf- und Traumzustände der menschlichen Seele*, p. 213. Calkins, this JOURNAL, V, p. 319. Etc.

²See Calkins, *l. c.*, pp. 331, 2. Wundt, *Phys. Psych.*, II, p. 540.

³Even so, it is probably seldom found "pure." Cf. Calkins, *l. c.*, pp. 333, 4. I am not referring in the present paper to dreams induced by drugs.

two forms will come the dreams which result from auto-suggestion, from the "will" to dream or not to dream in a particular manner.¹ Here the validity of the volition will be altogether dependent upon circumstances.

Most of our dreaming is in terms of vision. Auditory dreams, especially those in which the auditory ideas are verbal, probably stand high in the order of frequency. Tone-dreams seem to be of rare occurrence. I have certainly dreamed in tonal ideas: *e. g.*, the *Preislied* in the *Meistersinger*. But I have no record, and autosuggestion has failed to induce a musical dream.² Dreams in terms of touch appear to be usually colored by cutaneous pleasure-pain, generally pain.³ Temperature ideas are not uncommon.⁴ Dreaming in terms of the organic sensations is, perhaps, only surpassed in universality by visual dreaming. We have dreams involving the respiratory sensation complex (suffocation, flight, etc.), the static sense (looking or falling from a height, etc.), sex, sensations from stomach and intestines, from the bladder (dreams in which the idea of water plays a part), and from the heart, muscular and movement complexes (resistance, fatigue, etc.), and so on.⁵ Such dreams are subject to a very curious objectification, which usually takes the form of translation into sight or hearing.⁶ Of course, in most instances, the dream ideas of the less frequent senses are found together with the more common visual or auditory ideas. Tones are sung by some person seen, heat is sensed amid certain visual surroundings, etc.—Taste and smell remain.

Wundt remarks that dream hallucinations of taste and smell occur but seldom.⁷ One reason for this is, probably, the difficulty of taste reproduction. "Memorial images of taste impressions are complications, in which the taste sensation proper is of but minimal intensity. It can be altogether replaced by movement sensations, for the reason that these (correlates of movements of mimetic expression) differ for different taste stimuli."⁸ So, too, the memorial representation of smell is composed principally, if not exclusively, of three disparate factors: the visual image of the odoriferous object, the sensation of movement in the nose (inspiration), and the

¹Cf. Nelson, this JOURNAL, I, p. 376.

²Wallaschek, *V. f. Mus. Wiss.*, 1892, pp. 233 ff.; Wundt, *Vorlesungen*, I, c.; Calkins, pp. 319, 322.

³Cf., *e. g.*, the cases cited by Ladd, *Psychology*, 1894, p. 412.

⁴In my own experience. Cf. Calkins, p. 319.

⁵Scherner, *Das Leben des Traumes*, p. 187.

⁶Wundt, *Phys. Psych.*, II, p. 539.

⁷In der Regel fehlen. *Vorlesungen*, p. 358.

⁸*Op. cit.*, p. 310.

touch-temperature complex occasioned by the inspired air. At the same time, this weakness of the true memory image furnishes no valid reason against the cropping up of the vicarious complex idea in the dream series. Taste-smell fusions form a fairly large part of waking conscious content; and the associative suggestiveness of smell impressions is well known.

Miss Calkins¹ found two gustatory presentation dreams in a total of 335 dreams; and four olfactory and no gustatory representation dreams in a total of 298 dreams. In the abstract of Professor Murray's paper, "Do we ever dream of tasting?" in the *Proceedings of the American Psychological Association*² there seems to be a confusion between the classification of dreams as presentative and representative (Calkins) and as illusions and hallucinations. The "representative" dreams include both illusion and hallucination ideas. A dream is no less a dream, because the peripheral sense organ is stimulated during sleep. As we have seen, "whether the central tract . . . can be excited by disturbances in the neighboring tracts without any peripheral stimulation" is a question which may be answered by a theoretical affirmative in the case of all the senses; but it is very doubtful whether, if we had accurate knowledge of the conditions, we should not find illusion to be the dream material in practically every instance; visual, auditory or what not. That is, it does not seem justifiable to single out the taste center as not centrally excitable, *because* it is so very easily excitable peripherally; the same holds of vision:— but there is every reason for supposing that the end organs of taste, like those of vision, are somehow concerned in the suggestion and formation of the dream idea.

During the present year I have collected five good cases of taste dreaming, no one of which is that of a presentation dream (Calkins).

1. On the evenings of January 22, 23 and 24 of the present year, I attempted to induce taste dreams by auto-suggestion. Every precaution was taken to avoid the occurrence of a presentation dream; the mouth thoroughly washed out, etc. There was no indigestion. The first two nights I was unsuccessful; but on the third a perfectly good taste dream occurred. It contained visual, auditory (speech), tactile, muscular (movement of self and others), temperature, affective (both pleasurable and painful), and conative elements, beside the gustatory. The taste dreamed of was that of

¹L. c., pp. 319, 321.

²New York, 1894, pp. 20, 21.

English school plum cake. All the elements of reproduction were present; the visual and motor idea of breaking off fragments from a slice, the tactile sensations from their crumbling in the mouth, taste and aroma. The dream continued beyond the taste part of it. Jotted down immediately after waking, the dream record comprised 300 to 350 words.¹ The dream was of the morning class.² It was, however, a true sleeping dream.³ I had arranged to be awaked somewhat earlier than usual, in order to prevent the confusion of dozing with sleep proper, and was on the morning of January 25 aroused from sound sleep. On waking, I had the normal saliva taste in the mouth, which appeared on re-testing to be perfectly free from food fragments. The associative connections between the dream and events of the waking life were traceable with rather exceptional completeness. This may have been due, in part at least, to the fact of autosuggestion.

2. The second instance appears also to belong to the class of suggested dreams. It was recorded by the Rev. A. Beede, Alfred, Me., who had seen a notice of Professor Murray's paper in the *Philosophical Review*, and "resolved to watch for an opportunity of verifying" the occurrence of taste dreaming in his own experience. The dream took place in the night of Feb. 15, 1894. The mouth was clean. There was, perhaps, a very slight indigestion. The dream contained visual, tactile, muscular (movement of self), affective (both pleasurable and painful) and conative elements, beside the gustatory. The taste dreamed of was that of fresh strawberries; two good, one over-ripe. Reproductive elements present were: picking of the fruit and placing it in the mouth, taste (pleasant and unpleasant) and aroma, two acts of swallowing and (?) one of spitting out. The dream continued after the taste part of it. The letter of communication contains 350 to 400 words. The dream was of the night class. On waking, the taste in the mouth was, perhaps, not quite normal; this, like the presence of slight indigestion, is doubtful. The associative connections between the dream and events of the waking life were traceable with very considerable completeness.

It does not seem necessary to transcribe the remaining three dreams in detail. Each dream is reported by a different observer; and I have every reason to believe the reports trustworthy. No one of the three was autosuggested.

¹Nelson, p. 399.

²Calkins, p. 318.

³Nelson, p. 353.

These five are all "representative" dreams; and with the possible exception of two, as "hallucinatory" as dreams can well be. If the evidence be still regarded as unconvincing, I would propose that trial be made of autosuggestion. This can, of course, be done without any lapse into that form of the "psychologist's fallacy," against which Professor Murray cautions dream observers.¹

Postscript. Since the above paper was sent in to the Editor, Sept. 14, I have received accounts of three more taste dreams; two from new observers. No one of them was suggested. Since that date, also, there has appeared Professor Ribot's article, *Recherches sur la Mémoire affective* (*Rev. phil.*, Oct., 1894), which confirms many of my arguments.

VII.

ON THE QUANTITATIVE DETERMINATION OF AN OPTICAL ILLUSION.

(Continued.)

BY R. WATANABE, PH. D.

On page 418 of the current volume of this JOURNAL, Mr. Knox writes, apropos of the dotted-line and point-distance illusion, as follows: "Binocular bisection of horizontal distances is not subject to any constant error; binocular bisection of verticals is subject to the constant error of overestimation of the upper part of the field of vision. We should, therefore, expect to find the *m. v.* of our vertical Δ 's greater than that of our horizontal. The results [do not verify this expectation] This is curious. We are unable to offer any explanation of the result."

Further experiments upon the illusion in question were made, in the hope of elucidating this difficulty. Every precaution that could be thought of was taken to ensure accuracy and avoid the intrusion of complicating factors. Mr. Knox' experiments were exactly repeated, with a single modification. Whereas, on his cards, the point-distance was constant, and the dotted-line variable, on our own the reverse was the case. We imagined that if this alteration in the nature of stimulus brought about any alteration in judgment, the latter would be of such a kind as to be readily determinable for itself; and that this determination, itself an interesting side issue, would not interfere with the realization of the main object of the new experiments. On the other hand, the stimulus altera-

tion might prove to be without influence upon the judgment process; in which case we should at least obtain the negative result.

Series were obtained from three subjects: Messrs. Knox (*K.*; see pp. 416, 419), Pillsbury (*P.*; see pp. 417, 419, 421), and Read (*R.*). The following tables correspond in every respect to those of pp. 415 ff. :

TABLE I.

Reagent *K.* Vision normal. Method (*e*) predominant. General and special practice. Unit = 1 mm.

SERIES.	<i>R.</i>											
	<i>C</i> -25	<i>n.</i>	<i>m.v.</i>	<i>C</i> -30	<i>n.</i>	<i>m.v.</i>	<i>C</i> -35	<i>n.</i>	<i>m.v.</i>	<i>C</i> -40	<i>n.</i>	<i>m.v.</i>
<i>C-V</i>	27.18	3	0.62	32.12	3	0.62	36.62	3	0.78	42.62	3	1.03
<i>V-C</i>	25.25	3	1.66	32.00	3	1.25	36.95	3	0.80	42.00	3	0.91
$\frac{V}{C}$	26.29	3	0.82	31.95	3	0.63	37.16	3	0.63	42.16	3	1.08
$\frac{V}{C}$	26.71	3	0.90	31.37	3	0.73	36.95	3	0.64	41.74	3	0.79
<i>Hor. Δ</i>	+1.21			+2.12			+1.78			+2.31		
<i>Vert. Δ</i>	+1.50			+1.66			+2.05			+1.95		

TABLE II.

Reagent *P.* Vision Normal. Method (*b*) predominant. General and special practice. Unit = 1 mm.

SERIES.	<i>R.</i>											
	<i>C</i> -25	<i>n.</i>	<i>m.v.</i>	<i>C</i> -30	<i>n.</i>	<i>m.v.</i>	<i>C</i> -35	<i>n.</i>	<i>m.v.</i>	<i>C</i> -40	<i>n.</i>	<i>m.v.</i>
<i>C-V</i>	25.08	12	0.81	30.89	11	0.56	35.78	12	0.66	40.70	12	0.79
<i>V-C</i>	25.40	12	0.69	31.65	12	0.58	36.58	12	0.80	41.92	12	1.06
$\frac{V}{C}$	26.48	9	0.57	32.84	9	0.65	37.81	9	0.72	43.60	9	0.91
$\frac{C}{V}$	26.44	9	0.86	32.40	9	0.68	37.37	9	0.85	42.61	9	0.83
<i>Hor. Δ</i>	+0.24			+1.27			+1.18			+1.31		
<i>Vert. Δ</i>	+1.46			+2.62			+2.59			+3.10		

TABLE III.

Reagent *R*. Vision normal. Special practice only. Method mixed.
Unit = 1 mm.

SERIES.	<i>R</i> .											
	<i>C</i> =25	<i>n</i> .	<i>m.v.</i>	<i>C</i> =30	<i>n</i> .	<i>m.v.</i>	<i>C</i> =35	<i>n</i> .	<i>m.v.</i>	<i>C</i> =40	<i>n</i> .	<i>m.v.</i>
<i>C</i> - <i>V</i>	27.10	6	1.27	31.39	6	1.60	37.26	6	1.39	41.89	6	1.17
<i>V</i> - <i>C</i>	27.49	6	1.08	32.83	6	1.32	37.53	6	1.48	42.49	6	1.41
$\frac{V}{C}$	27.37	6	1.27	32.29	6	1.51	37.66	6	1.57	42.41	6	1.47
$\frac{C}{V}$	26.72	6	1.37	31.25	6	1.33	36.83	6	1.22	41.74	6	1.33
<i>Hor.</i> Δ	+2.29			+2.11			+2.39			+2.19		
<i>Vert.</i> Δ	+2.04			+1.77			+2.24			+2.07		

*Remarks.*¹ (1) The illusion holds for every observer.

(2) Vertical Δ 's are larger than horizontal, in these twelve comparisons, in six cases; smaller in six. But in none of these contrary cases does the difference of the two Δ 's amount to half a mm. (Differences are: Table I—0.46, 0.36; Table III—0.25, 0.34, 0.15, 0.12.) Moreover, four of them come from the least practised reagent, *R*., who began with horizontal judgments. Mr. Knox' conclusion under this head is, therefore, confirmed by our results.

(3) The main object of the present investigation has been stated above. Do we find any light thrown upon the matter by the present figures? We have:

Table I. $r\ 0, = 2$ (.79, .63; .97, .93), $w\ 2$ (1.72, 0.86; .93, .68).

Table II. $r\ 0, = 4$ (.75, .71; .57, .66; .73, .78; .92, .87), $w\ 0$.

Table III. $r\ 2$ (1.17, 1.32; 1.29, 1.40), $= 2$ (1.46, 1.42; 1.43, 1.39), $w\ 0$.

In all, $r\ 2, w\ 2, = 8$. Mr. Knox obtained $r\ 6, w\ 7, = 7$; or, if his Table I be included, $r\ 8, w\ 8, = 8$. Massing, therefore, we get $r\ 10, w\ 10, = 16$. We do not insist upon the absolute relations of these figures,—apart from the fact that massing, even in two such comparable cases as these, is psychologically unjustifiable. Nor do we fail to note that of

¹See pp. 418 ff.

our own = 8, six have what tendency to differ they do have in the direction of w ; of Mr. Knox' = 8, seven have such a tendency. But we feel safe in formulating the following proposition: *so far as Mr. Knox' and our own experiments extend, there is strong evidence that, in presence of the dotted-line and point-distance illusion, the illusion of over-estimation of the upper half of the field of vision disappears*; the evidence being couched in terms of the $m. v.$ in vertical and horizontal quantitative determinations of the former illusion. We are not at present prepared to suggest any explanation of this fact. The fact itself holds, whether we employ the method with knowledge or the method without knowledge, and whether the reagent be practised or comparatively unpractised in *Augenmass* experiments.

(4) The values of $\frac{\Delta}{r}$ are:

I. Hor.: $\frac{1}{20}, \frac{1}{14}, \frac{1}{19}, \frac{1}{20}, \frac{1}{17}$. Vert.: $\frac{1}{17}, \frac{1}{18}, \frac{1}{17}, \frac{1}{20}, \frac{1}{21}$.

II. Hor.: $\frac{1}{104}, \frac{1}{23}, \frac{1}{24}, \frac{1}{29}, \frac{1}{30}$. Vert.: $\frac{1}{17}, \frac{1}{11}, \frac{1}{12}, \frac{1}{13}, \frac{1}{14}, \frac{1}{15}$.

III. Hor.: $\frac{1}{11}, \frac{1}{14}, \frac{1}{14}, \frac{1}{15}, \frac{1}{19}, \frac{1}{20}$. Vert.: $\frac{1}{12}, \frac{1}{10}, \frac{1}{17}, \frac{1}{15}, \frac{1}{16}, \frac{1}{19}, \frac{1}{20}$.

(a) Table I is taken from the same reagent as Mr. Knox' Table III. The values of its limina are probably vitiated for the reason alleged on page 419. (b) Table II is taken from the over-practised reagent *P*. (See page 419.) Here, as in Mr. Knox' Table IV, the vertical Δ 's have suffered much less by practice than have the horizontal. (c) Table III, from a previously unpractised reagent, confirms Mr. Knox' general formula (p. 419) a good deal better than his own Table I, from a similar reagent, does. (d) The valuelessness of $C = 40$ mm. is indicated by Table III. We should not expect to find evidence of it in the other two tables. It is, perhaps, hardly necessary to make the explicit statement that these supplementary experiments were not at all ex-

pected to throw light on the magnitude of $\frac{\Delta}{r}$. The two reagents *K* and *P* were wholly unsuitable for such a purpose. On the other hand, the reasons that disqualify them for that investigation do not come into account for the main issue, discussed under (3). And it is, at least, satisfactory to note that there is nothing in the fractions which makes against Mr. Knox' conclusions. Thus, Tables I and II alike make the vertical $\frac{\Delta}{r}$ greater, on the average, than the horizontal; and the variation from this rule, in Table III, is so slight as

to be readily explicable in terms of the order of special practice (*cf.* Table I, p. 419). (*e*) Did the reversal of the *C* and *V*, as compared with those of Mr. Knox' investigation, influence the process of judgment? (i) The opinion of the reagents *K.* and *P.* was to the effect that it did not. (ii) If we compare the fractions obtained by the old and new methods, we find that those of the latter case are :

Table I. *Hor.* : $\langle \langle \langle \rangle \rangle$; *Vert.* : $\langle \langle \rangle \langle$,

Table II. *Hor.* : much $\langle = \langle \langle$; *Vert.* : \langle just $\langle = =$, as compared with those of the former. This general lessening was to have been expected, other things equal, from the increase of practice. Had the interchange of *C* and *V* had any influence, it would, we think, have been one in opposition to this tendency to lessen. For *a priori*, if there is any question of relative ease or difficulty, it should be easier to estimate when the dotted line varies (Mr. Knox' procedure) than when the point distance is the variable (our own). In the former case, an extension difference carries with it a quality difference, a greater or less number of dots: while, when the extension of the point-distance alters, no qualitative change is involved. The fact that the practice-lessening of the fractions is so little counteracted, therefore, in our results, tends to confirm the verdict of introspection. (iii) Moreover, the horizontal figures of Table III show, as has been pointed out, a very good agreement with Mr. Knox' formula. On the whole, then, we would answer the question of this paragraph in the negative. (*f*) Since the appearance of Mr. Knox' paper, there has been published in the *Zeitschr. f. Psych. u. Physiol. d. Sinnesorg.*, an attempt at a quantitative treatment of another optical illusion—that of the arrow head and feather (*F. Auerbach: Erklärung der Brentanoschen optischen Täuschung*, Vol. VII, pp. 152 ff.). The numerical results (p. 159) are not comparable with those given in the two present papers; the point investigated being not the quantitative variation of the illusion with variation of absolute magnitude of lines or distances, but its increase with increasing length of the limbs of the limiting right angles. But attention may be called to certain remarks of the writer's, bearing upon the general question. (i) The illusion varies with the *visual habits* of the reagent (p. 155). We have had no opportunity of testing this, in Auerbach's way. But the statement receives indirect confirmation from Mr. Knox' conclusions, p. 419 of this JOURNAL. (ii) Increased concentration of vision and attention diminishes the illusion (p. 155). This holds of the arrow head and feather illusion, for the explanation of which the influence of indirect vision is called into account, to a greater extent than for our own.

At the same time we have seen that a similar result may be obtained by familiarity and practice. (iii) It is necessary, for determination of the limen of difference, to avoid knowledge of the actual relations of the distances compared on the part of the reagent (p. 159). This point has also been insisted on by Mr. Knox. (iv) Judgment should be as immediate as possible; since it is apt to fluctuate, if the stimulus is present for any length of time (p. 159). Cf. the length of Mr. Knox' experimental series (p. 414). The method employed both by Auerbach and ourselves being a form of minimal changes, the necessity of immediacy of judgment is a matter of course.

VIII.

THE CUTANEOUS ESTIMATION OF OPEN AND FILLED SPACE.

BY PROFESSOR C. S. PARRISH.

A comparison of the spatial functioning of the cutaneous and visual sensibilities must always possess an especial psychophysical interest. The study of sensational intensities culminates in Weber's Law; that of sensational quality leads to a whole number of alternative psychophysical theories; the determination of the temporal attributes of sensation is one means of approaching the problems of the so-called time-sense; that of its spatial attributes, the first step towards a psychological space construction. But, whereas every sensation is possessed of duration, quality, and (with the exception of the visual series) intensity, a space attribute attaches exclusively to the sensations of sight and pressure. This fact, which seems at first sight to simplify the space problem, in reality renders that problem unusually difficult of solution.

Opinions differ very widely as regards the sensational factor in psychological space, as regards the interaction of eye and skin in its construction, and as regards the attributes and aspects of the cutaneous sensibility itself. It may, therefore, be well to give here, at the outset, a brief *credo*, not with any intention of dogmatizing, but merely with a view to clearness and intelligibility.

We believe, then, that the development of the eye, as a space organ, far outran that of the skin. That tactual space was, accordingly, built up under the influence of, and remains almost invariably subject to that of vision. Nevertheless, that there are two psychological spaces, and not one space. We consider, further, that the mechanical cutaneous sensi-

bility has (*pace* Dessoir) only one quality, that of pressure. That the cutaneous local signature is physiological only, although the retinal—as is indicated by many facts which cannot here be adduced—is psychological. And that in its ordinary functioning, the skin co-operates with the three deeper lying sensibilities, the tendinous (with its quality of strain), the articular (with its quality of pressure, and possibly with its own local signature), and the muscular (with its peculiar quality which only becomes seriously involved in fatigue or exhaustion). Finally, we ascribe to the cutaneous and visual sensation an attribute of extension, which we regard as co-ordinate with intensity, quality and duration, and which is by no means to be confused with the “bigness” or “massiveness” predicate of one school of nativistic psychologists.

After this preface we may approach the special problem which heads this paper. Almost unexceptionally, the eye regards a filled space as greater than an empty space objectively equal to it. What is the attitude of the skin to such spaces? It has recently been maintained that pressure *plus* movement functions, in this regard, as does the eye.¹ But one of the explanations propounded for the visual illusion is couched in terms of movement: it being argued that though the resting eye is also subject to the error, it is only so subject because, at some time or another, it has moved. The argument is paralleled by many others of the chapter of psychology which deals with visual perception, and need not be further commented on. Now, if the resting skin (*sit venia verbo!*) were deluded equally with the moving, then, although visualization might be called in to explain the fact, we should still be in presence of a phenomenon telling with more or less of force against the movement theory. If, however, the resting skin, in spite of visualization, and in spite of its own constant movement in the past, should prove to be not deluded, the movement theory is so far supported. Should the illusion be actually reversed, we must look for the conditions of such reversal in the special psychophysics of the organ.

The problem, then, resolves itself into that of obtaining comparative space judgments from the resting skin. If the skin, and the skin only, is to be appealed to, stimulation must be liminal. In this case, however, judgment will be uncertain, and comparison difficult. Since in ordinary life the static functioning of the skin is almost invariably correlated with a similar functioning of the deeper lying sensibilities—since, *i. e.*, normal stimulation is almost always supra-

¹Dresslar, this JOURNAL, pp. 332 ff. The view of this author is criticised below.

Method.—We imagined, at the outset of the investigation, that a very large number of experiments would be needed, if any satisfactory result was to be obtained. We therefore employed a modification of the method of right and wrong cases. Instead of taking too little-different stimuli, as that method requires, we proposed to compare each block with every other block, recording the judgments as *r*, *w*, = and ? [*r* being used on the optical analogy, when the cutaneous judgment made a filled larger than an objectively equal empty space]; while, to avoid *Einstellung*, we did not confine ourselves to one pair of blocks in each series of experiments, but intermixed the comparisons at random. But the uniformity of which we were in search made its appearance so quickly, decidedly and unmistakably, that this original plan was not carried out. We have, consequently, only a relatively small number of experiments taken by this method, and those we propose to submit in detail.

In the following Table the first column gives the blocks compared, the numbers (as stated above) signifying the number of points in each block; while in the others, each of which is accredited to a different reagent, the signs ($>$ or $<$) gives the judgment of relation recorded *N* times out of *n* experiments. Thus, " $2:3 > 4:6$ " means that the two-point distance was judged greater than the three-point distance four times out of six experiments, by the particular reagent. Of the seven subjects, one only, (*Ti.*) had had general as well as special practice. His results, although not numerous, are the most reliable. The other six reagents were specially practised for the purposes of this investigation. It should be stated that during the first half (approximately) of these experiments, the application of the second stimulus was not to the exact part of the skin stimulated by the first, but to a line just alongside of it; during the second half the successive applications were made at precisely the same place. Absolutely no difference in result could be discovered; and irradiation makes this intelligible. Both sets of experiments have, therefore, been drawn upon in the composition of the Table.

Remarks.—(1) We notice at once that, for the resting skin, a filled distance is, on the average, shorter than an empty distance objectively equal to it. This holds for every reagent. In the earlier stages of practice, when visualization was especially insistent, there occurred sporadic cases to the contrary effect: the filled distance appeared longer. But such cases disappeared as practice progressed; and introspection referred them very definitely to the influence of the visual idea.

TABLE I.

	B.	Ha.	H.	O.	Ol.	T.	T ₂ .
	N ⁿ	N ⁿ	N ⁿ	N ⁿ	N ⁿ	N ⁿ	N ⁿ
2 : 3	> 4 6	> 2 2	> 2 4	> 5 6	> 4 5	> 7 11	> 4 4
: 4a	" 5 6	< 4 5	" 7 13	" 1 2	" 5 6	" 5 8	" 2 2
: 4b	" 3 4	—	" 3 4	" 5 5	" 4 5	" 3 3	—
: 5	" 4 6	> 5 6	" 4 6	" 5 7	" 3 5	" 5 7	" 5 6
: 6	" 3 4	" 3 3	" 4 6	" 4 4	" 3 3	" 4 6	" 2 2
: 7	" 2 4	" 3 4	" 3 4	" 4 4	" 2 3	" 9 10	" 2 2
: 8	" 4 6	" 1 2	" 5 5	" 6 7	" 5 5	" 3 4	—
: 9	" 2 4	" 2 2	" 4 4	" 5 6	" 4 4	" 4 4	—
3 : 4a	" 3 4	< 4 5	" 3 5	" 2 4	" 5 6	< 4 4	—
: 5	" 6 9	> 4 6	" 3 4	" 6 8	" 4 4	> 5 8	" 4 7
: 6	—	" 3 4	" 3 6	" 6 8	" 3 4	" 5 6	—
: 7	" 1 2	" 7 8	" 4 8	" 5 5	" 5 7	" 8 11	" 4 4
: 8	" 1 2	" 3 5	" 3 6	" 2 4	" 3 4	" 4 5	" 2 2
: 9	" 1 1	" 3 6	—	" 2 3	" 2 2	" 6 8	" 3 4
4a : 5	" 4 7	" 2 3	" 2 4	" 3 3	" 2 4	" 1 2	—
: 6	" 1 2	" 1 2	" 4 6	" 6 8	" 2 3	" 3 4	—
: 7	" 5 6	" 4 6	> 5 7	" 3 3	" 2 2	" 4 8	—
: 8	< 3 4	" 1 2	" 5 8	" 4 4	" 3 4	" 3 4	" 2 2
: 9	> 2 3	" 1 2	—	" 2 3	" 2 3	" 2 4	—
4b : 5	—	< 2 2	—	" 2 3	" 2 3	" 2 4	—
: 6	—	—	—	" 3 4	" 2 4	" 2 3	—
: 7	—	—	" 2 3	" 2 3	" 2 3	" 4 4	—
: 8	" 1 2	—	< 1 2	" 2 3	" 2 3	" 2 3	—
: 9	—	—	" 2 3	" 2 3	" 2 3	" 2 4	—
5 : 6	" 2 3	" 2 2	> 3 4	" 2 3	" 2 3	" 2 2	—
: 7	" 5 8	> 4 5	" 2 3	" 5 6	" 3 4	" 7 7	" 3 4
: 8	" 1 2	> 3 4	" 3 5	" 5 8	" 1 2	" 2 3	" 2 2
: 9	" 2 4	> 2 4	" 1 2	" 3 3	" 2 3	" 6 8	" 5 6
6 : 7	—	" 1 2	" 2 2	> 2 3	" 2 3	" 3 4	" 2 2
: 8	—	—	" 2 2	" 3 4	" 2 3	" 4 4	—
: 9	—	—	" 3 4	" 2 3	" 2 3	" 3 6	—
7 : 8	—	" 2 4	—	" 2 3	" 2 3	" 4 6	—
: 9	—	< 2 2	—	" 3 6	" 2 3	" 4 7	—
8 : 9	—	—	—	" 4 8	> 2 4	" 4 5	—

(2) There are a few exceptions to this rule. To understand them we must ask at once—taking the confirmatory evidence of Series II for granted—for the reason of the reversal of the illusion, in its transference from optics to haptics. That reason is given in terms of introspection by the reagents. All alike asserted that the points in the filled line were sensed as "bunched" or "crowded" together. The space between two points can be fairly accurately apprehended—we are not speaking, of course, of objective accuracy—but a space which is more or less filled "shrinks" together, and may be reduced to what are comparatively very small proportions. Doubtless, for the majority of the subjects, the first judgment was more influenced by visualization than was the second. But this is

an error common to almost all cutaneous experiments. And, though it may have aided the reversal of the illusion, it certainly could not have produced it. Moreover, the reagent *Ti.* has been able, by practice, pretty completely to separate the visual from the cutaneous judgment,¹ and the reversed illusion holds quite strongly in his case. The "bunching" or "crowding" is psychophysically explicable in terms of irradiation.

(3) Now for the exceptions. (a) $2 < 4a$ for *Ha.* Reference to the scheme will show that $4a$ may be regarded as an open line, doubly bounded at either extremity. This double bounding would make the point-impressions especially intensive. Now it proved to be a constant error in these experiments—one which evidenced itself in the practice series, in which experimenter was being educated as well as experimentee—that *increased intensity of pressure meant a judgment of increased length of line.* We did not attempt any quantitative evaluation of this error; the error itself was eliminated by the acquisition of facility and accuracy in the handling of the blocks. But we suggest that the error, in a modified form, may account for these exceptional judgments. (b) $3 < 4a$ for *Ha.*, *H.* and *T.* This is, again, easily accounted for. 3 is a filled line; $4a$ may be sensed as a doubly bounded open line. When this is the case, the judgment $<$ will follow, in terms of the cutaneous illusion. (c) $4a < 6$ for *Ha.* This we can only explain by supposing that the dots in the middle of 6 were crowded together in sensation, the terminal points being thus left free. (d) $4a < 8$ for *B.* Again, we have possibly a similar explanation. The two 4's of 8 are crowded, leaving the centre space free; $4a$ is more irregularly filled for sensation. Introspection gave a confirmatory result in both these cases. (e) $4b < 5$ for *Ha.* The two blocks are so similar, that any accidental and variable factor may have conditioned this judgment. (f) $4b < 7$ for *O.* and *Ol.* Here, again, the blocks are of like patterns. Perhaps the central dots of 7 were massed, leaving the ends free. (g) $4b < 8$ for *H.* and *T.*; cf. (d) above. (h) $5 < 6$ for *Ha.* The former is more uniformly filled. (i) $5 < 8$ for *Ha.*, *O.* and *Ol.*; cf. (d) and (g) above. (k) $5 < 9$ for *O.* This cannot be explained by reference to the blocks. (l) $7 < 9$ for *Ha.* and *Ol.* Nor can this.

We would call attention to the facts: (i) that these are very few exceptions, *when the fewness of the experiments in*

¹Proof of this statement will be advanced later, in articles by Dr. M. F. Washburn and Mr. W. B. Pillsbury, dealing respectively with the influence of the visual idea upon cutaneous space judgments, and upon cutaneous localization.

general is borne in mind; (ii) that most of them are explicable in terms of the illusion itself; and (iii) that they are by no means co-ordinate. For the discrimination of certain of the blocks, pretty thorough *practice* and very constant *attention* are necessary. Judgment becomes at once uncertain if *fatigue* has begun to set in. Not only is an accidental increase of pressure liable to be interpreted as an increase of length, but *vagueness or insecurity of judgment* (due to exhaustion, inattention, etc.) was also found to be so interpreted. Yet, in face of the fewness of the experiments and of all these sources of error, we see that 2 is judged greater than every other block, except in one set of judgments from one reagent, with the misleading block 4a. We may remark, also, that the evidence from the experiments is stronger than that from the table; since there occurred cases—not many, it is true—in which the judgment contrary to $>$ is not $<$, but $=$. Such cases have not been specially treated by us.

SERIES II.

In the second series of experiments a line was compared with a point-distance. The line was the impression obtained from the application of a strip of hard rubber, $\frac{1}{100}$ inch in thickness. The point-distance was given with the aesthesiometer figured on p. 422; the bulb being left unemployed, and the pressure regulated by practice. This was necessary, since we had no rubber strips, but only points, attached to the shaft of the instrument. The method followed was that of right and wrong cases. Here, again, the experiments, though not numerous, speak with complete decisiveness for the reversal of the optical illusion.

The first column of the table gives the reagent; the second, the length of the filled line, in mm.; the third the point-distances with which it was compared—the difference between each point-distance and its next successor being 1 mm.; the

TABLE II. Unit = 1 mm.

R	Line.	Limits of Pt.-distances.	n.	Equivalence.
B.	28	28—23	15. 20. 20. 20. 40. 40.	Between 24 and 23
Ha.	20	20—15	30. 15. 35. 30. 35. 100.	15
T.	28	28—24	20 throughout.	Between 25 and 24
O.	28	28—23	17. 20.	" 24 " 23
Ol.	28	28	20 throughout.	" 23 " 22

fourth, the number (n) of experiments made with each point-distance; the fifth the point-distance which proved to be subjectively equal to the constant line-stimulus.

I. e., an open space of 24 mm. is equal to a filled line of 28 mm., and one of 15 to a filled line of 20. The different values of the line were taken owing to the fact that the limen of twoness for *B.*, *T.*, *O.*, and *Ol.* lay considerably higher than for *Ha.*

Table III shows the results of lines 2 and 5 of the above Table more in detail, and proves the point made just now—that the experiments speak very decidedly for the reversal of the optical illusion.

TABLE III a.

TABLE III b.

Reagent <i>Ha.</i> Rubber line = 20 mm.			Reagent <i>Ol.</i> Rubber line = 28 mm.		
Point Distance.	Judgment.	Per Cent.	Point Distance.	Judgment.	Per Cent.
20	>	88	28	>	100
19	>	83	27	>	97.5
18	>	85	26	>	90
17	>	75	25	>	90
16	>	80	24	>	77.5
15	=	88	23	>	77.5
			22	<	72.5

Further experiments might still further regularise the per cents.; they could hardly do more.

Conclusion. Literature.—We think the conclusion to be pretty obvious, that for the resting skin a filled line is shorter than an open space objectively equal to it. We have already suggested an explanation of this fact, in terms of irradiation, and specially directed visual association.

It has been asserted, quite recently, by Mr. Dresslar, that the illusion for touch is identical with that for sight. We would offer the following remarks: (*a*) In Dresslar's experiments it was not an open space that was compared with a filled, but a uniformly filled space (surface of a smooth card) which was compared with a discontinuously filled space (punctured card). It will be necessary to make experiments, both on active and passive touch, in this way: that a really open space be compared with a discontinuously filled

space. Till this has been done, the work of this experimenter must remain equivocal. (b) In active touch, the deeper lying sensibilities are involved to a much greater extent, and much more definitely from the qualitative point of view, than in our own experiments. We find no reference to this fact in Dresslar's introduction. (c) Whether we experiment with active or passive touch, the perception of movement is implied, and this is of itself amply sufficient to arouse the visual analogy. It seems hardly credible that Dresslar should not have come upon the visualization error in the course of his investigation. But we have not found any reference to it in his article. (d) Even for touch, as distinguished from pressure, there is evidence against Dresslar's results. James (*Principles*, II, p. 250—wrongly quoted by Dresslar as 242) declares that if the finger-tip be moved over a smooth and punctured card surface, the distances being objectively equal, the *filled* (i. e., punctured) *space is shorter*. Loeb (*Pflüger's Arch.*, XLI, p. 122—quoted by Dresslar as 121), it is true, found that an unevenly coated wire, drawn between finger and thumb, appeared longer than a smooth thread of equal length but somewhat less diameter, drawn at the same velocity. Loeb, however, calls especial attention to the effect of friction in this case. While therefore, his conclusion so far confirms that of Dresslar, the two experiments are not strictly comparable, nor is Loeb's at all exact. James is characteristically deficient in his description of his own experiments, but, so far as these are reliable, they stand in direct contradiction to Dresslar's. (e) A confirmation of our own results for the resting skin will be found in James' *Principles*, II, pp. 141, 142. The experiment cited is, however, very rough, and no numerical determinations are given. (f.) Dr. Nichols also supplies confirmation in his book, *Our Notions of Number and Space*, pp. 97, 105, 106, etc. . . . We believe that this investigation, as well as that of Mr. Dresslar, must be very carefully scrutinized and tested, before its results can be accepted. But this is not the place to enter upon a general criticism.

So far as it goes, our own conclusion, that for the resting skin the filled line is shorter, distinctly supports the theory that the key to the corresponding optical illusion is to be looked for in movement. It will be interesting to see whether more accurate experiments than those quoted upon active and passive touch confirm this view, or point to the necessity of its modification.

[Both of the above sets of experiments were well under way before Mr. Dresslar's paper appeared. Dr. Nichols' book, received by the *Philosophical Review*, August 4, was not seen till after their conclusion.—E. B. T.]

NOTE TO STUDY NO. V.

1. Certain correspondents, among whom is my friend Dr. Meumann, of Leipzig, have pointed out what they judge to be a defect in the Washburn aesthesiometer (see p. 422), and what must also be regarded as a defect in the very similar Jastrow model. It is this: that there is no guarantee of the simultaneity of the two impressions. I would urge that even if this be granted, the new instrument is better than the old, for it at least ensures constancy of pressure, which that did not. But I think that more can be said in its defence. Its form makes it easier to handle; and pressure made by it can be more readily controlled as regards the time factor, the attention not being distracted by the necessity of pressure regulation. And it certainly does not lie in wait for the experimenter with a constant error, as the sensibilmeter does. If the skin, in the place worked on, is perfectly flat or of symmetrical curvature, the bulb might with advantage be held in a fixed support, and the part played by the hand be confined to that of regulating the time of impression and of release from stimulation. But where this is not the case, regulation of simultaneity by hand seems desirable. I do not know of any other control than those of the vision of the experimenter, and introspection of the experimentee. Of course the rubber points could be made to pass through the bar that holds them, and their length as regards one another be regulated by the skin curvature in the experimental series; but this alteration would introduce one of the faults of the sliding-scale form of the instrument—and would not the controls, after all, be then precisely what they are now?

2. Dr. E. W. Scripture and myself have devised an improved arm-rest for the new model kinesimeter. Cuts of this and of the instrument itself will appear in the next number of the JOURNAL.

E. B. T.

THE DAILY LIFE OF A PROTOZOAN: A STUDY IN COMPARATIVE PSYCHO-PHYSIOLOGY.

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AND

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The life of an animal, as we attempt to study its physiology, appears to consist of a fabric of interwoven rhythms. Circulation, respiration, daily activity and rest, as well as reproduction and to all appearances many processes of nutrition, muscular contraction and the transmission of nerve impulse, all have come to take the form of waves or rhythms, which differ greatly in period and at certain points are interdependent. The waves of external nature, into the midst of which an animal's life is cast, no doubt tend to cause rhythmic responses on the part of the animal. This is well exemplified in the evident relation between seasonal and lunar periods and reproductive rhythms, and in the rest or activity of day and night. That external changes are not the sole determinants of physiological processes is amply demonstrated, however, by the fact that different animals living under similar environment, possess widely different rhythms. Certain controlling factors must, therefore, be sought for within the animal itself, and clearly these must be closely related to physical structure and endowment. In other words, we could not expect an organism to respond to stimuli unless it possesses mechanisms by which the stimuli may be felt. Or, conversely, if an animal responds to changes in external nature we must suppose the existence of mechanisms for their perception, although specialized structures may not have been demonstrated. To ascertain to what extent physiological processes are in fundamental character rhythmical and to be able to learn approximately what normal rhythms are, will require continuous observation of a series of animals, each for a considerable period. A series of such observations would naturally begin with the simplest animals, the protozoa.

While taking other rhythms into careful account, the present research had for its primary object a study of the rhythm of rest and activity in one of the protozoa. Since in these simple organisms, consisting of but a single cell, there are found all the important physiological, and, for all we are able to observe, types of all the psychic processes which take place in the life of one of the higher animals, it would seem possible to observe directly all the steps in the fatigue of gland, muscle or nerve which are demonstrated in more complicated bodies by indirect methods. We should be able to see, during a period of rest, zymogen granules forming and being stored up, the body grow, and possibly the nucleus increase in size. Following this, if the life of a protozoan is similar to that of higher animals in these respects, we should have a period of activity, in which new food is secured, while the body is emptied of its formed materials. If the life of a protozoan is found to be cast on rhythms similar to those of higher animals, the fact will be most remarkable, since their physical equipment is so different. If protozoan rhythms prove to be strikingly dissimilar, in what ways may such peculiarities be correlated with differences in structure?

The work here described was done in the physiological laboratory of Clark University, in the fall of 1893. For much assistance in construction of apparatus, we wish to express our thanks to Mr. J. R. Slonaker.

Among the numerous protozoa available, the *Vorticella*¹ was chosen for two reasons. First, being attached permanently by its stalk, a specimen can be retained in the field of a microscope for days without difficulty. This is, of course, a prime condition of the experiment. Second, the animal is active, its movements are well defined and easily observed and some of them appear to be clearly automatic, others, purposeful and selective. These movements may be classified as follows:

AUTOMATIC.

1. Contraction of vesicle.
2. Ingestion of food balls.
3. Ejection of detritus.

PSYCHO-REFLEX.

1. Contraction of stalk, with attendant closure of bell.
2. Vibration of peristomal cilia.
3. Sorting of particles by the the sensory cilia, the driving of food toward the mouth, and the driving away of waste particles.

¹Descriptions of the *Vorticella* are so readily accessible in all manuals of biology and zoology that no attempt to describe it here is deemed necessary.

Objection may be raised to placing ciliary activities upon the psychic side, and it is true that the action of cilia in various parts of the human body could not be considered of such character. On the other hand, the work of the cilia in Vorticellæ seems to be at every point more complicated than that of ordinary cilia. By their movements currents are set up with the apparent purpose of drawing food within reach. When a particle is touched by the cilia, an act of choice is apparent, and in accordance to this choice the particle is carried toward the mouth or whirled away. This process would seem to indicate no less conscious wakeful action on the part of Vorticellæ than the seeking of prey and the feeding of animals in general. No account is taken of the extension of the stalk, since on the view that this is due entirely to elasticity of the cuticle it is a purely mechanical action.

Apparatus was devised to record the occurrence of all these phenomena. It consisted of a continuous-roll kymograph with eight capillary glass pens arranged vertically across the paper. The lowest pen was connected with an electromagnet and the hands of a clock so as to register hours and minutes. Two other pens registered temperature and barometric pressure, and the five remaining were attached to tambours in such a way that the observer could record various activities by a touch of the fingers of the right hand, without taking his eye from the microscope. The first recorded contractions of the stalk; the second, contractions of the vesicle; the third, ingestion of food particles; the fourth, ejection of detritus; the fifth, reproductive phases. The observer's left hand remained free to adjust the focus of the microscope.

The microscope used was a Zeiss, apochromatic series, ocular 6, objective 4 mm., which gave a magnification of 375 diameters. In order to keep the Vorticellæ under conditions as normal as possible, a stream of water from an aquarium, in which various plants were growing, was kept flowing under the cover-slip. This was accomplished by means of a glass syphon, drawn to a capillary point, placed at one side of the cover-slip, and a filter-paper drip applied to the other side.

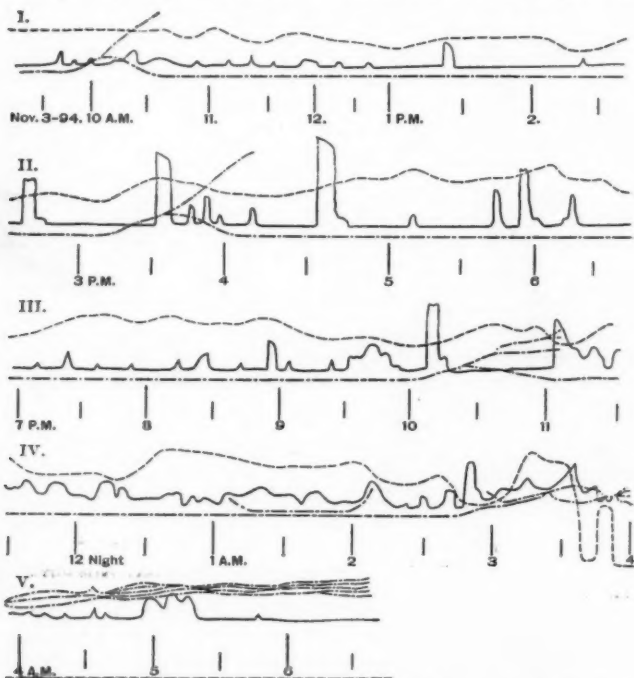
The first Vorticella observed corresponded to the species *gracilis*, as indicated in W. Saville Kent's "Manual of the Infusoria." It was observed, without a moment's intermission, between 8 and 8.30 and between 9.30 A. M., Nov. 3 and 6.30 A. M., Nov. 4, and every contraction of the stalk or vesicle registered upon the kymograph paper. The following table shows its life history for the whole period of observation. The times given in the first column are usually

TABLE.
Experiment 1. *Vorticella gracilis*.

Date.	Time.	Contraction of Vesicle per min.	Contraction of Stalk.	Remarks.
Nov. 3,	8.00 A. M.	3	8 in 30 mins.	No rhythm.
"	8.30 "	2.8	1 in 8 (about).	Division begins.
"	9.30 "	2.6	1 in 8 (about).	Two vesicles visible.
"	10.00 "	2.6	1 per min.	Division complete.
"	10.13 "	2.6	Several in rapid succession.	
"	10.33 "	2.6	Less frequent and less violent after division.	
"	10.35 "	2.6		
"	11.35 "			
"	11.55 "	2.6		
"	12.00 M.	3.2	Rare.	
"	12.15 P. M.	2.4	None.	
"	12.52-1.50	2.4		
"	2.00 P. M.	2	Several in rapid succession.	No stimulus apparent.
"	2.50 "	2.4	None in 25 mins.	
"	3.15 "	2.2	2.8 per min. (about) for 5 mins.	Second division begins.
"	3.33 "	2.8	1.5 per min. (about).	Two vesicles visible.
"	3.42 "	2.8	1.5 per min.	Division complete.
"	4.11 "	2.8		No observable stimulus.
"	4.50 "	2.8		Gas light. No visible change in contrac- tions of stalk or vesicle.
"	5.00 "	Slowly varying between 2.2	Varying irregularly, rare.	
"	to			
"	10.00 "		None.	Third division begins.
"	10.21 "		None.	Subdivision of daughter cell begins.
"	10.35 "		None.	Further subdivision of daughter cell begins.
"	11.00 "		Feeble contractions.	Daughter cells detached.
"	11.05 "	3.2	Feeble contractions.	Free-swimming zoid in contact.
Nov. 4,	1.15 A. M.	2.7		Vesicle much swollen.
"	2.10 "	2.4		Free-swimming zoid becomes attached.
"	2.12 "	1.6		
"	3.06 "	1d8		Vesicle suddenly divides.
"	3.15 "	3		Free-swimming zoid all break into one which con- tracts five or six times, at the rate of 1.5 per minute. After this no more
"	3.25 "			contractions of vesicle can be seen. Following this a curious succession
"	3.46 "	1.5		of partial divisions and reunions occurred, lasting until 6.30 A. M. See Figure.
"	3.52 "			Last contraction of stalk.
"	5.44 "			

those at which greatest changes appear in the rhythms recorded.

The course of this *Vorticella*'s life may be seen graphically represented in cut below. The lines in this figure are plotted directly from the kymograph record, but to avoid complication the coördinates, excepting designations of time in hours and half hours, have been omitted. The continuous line represents stalk contractions. For somewhat more than half its extent it is at its lowest level, indicating that no contractions occurred during that time. Elevations are drawn proportionate in height to the number of contractions occurring during the time traversed. The broken line indicates in a similar way frequency of vesicle-contractions, the higher the



Experiment 1, *Vorticella gracilis*. The continuous and broken lines represent frequency of stalk and vesicle-contraction respectively. The line of dots and dashes indicates reproductive phases. For further explanation, see Table above.

curve, the more frequent the beats. It reaches its zero point when the vesicle ceases to contract. The line of dots and dashes represents reproductive phases, its rise and division indicating increase in size and division of the Vorticella. The time at which one of the bells detaches itself from the stalk upon completion of divisions is marked by the termination of the corresponding line. Conjugation is indicated by the approach and union of a similar line.

No account has been taken of movements of the cilia, for the simple reason that they are not a variable in the animal's life. During the whole period of observation the cilia were working incessantly, drawing particles toward the mouth, sorting them, ingesting food and driving away excreta and debris. An apparent exception to this statement occurs at the instant of a stalk contraction, when the bell is also contracted into a sphere and the cilia are drawn in; but during this operation there is only time for the cilia to fold in and open out again, and hence there is nothing which could be construed as a period of rest. The same statement applies to the results of ciliary activity, viz., ingestion of food and ejection of detritus. In other words, so far as our observation goes, there is practically continuous action of cilia and, in consequence, a practically constant stream of particles both into the body and out from it.

As stated above, the temperature and barometric pressure were traced upon the kymograph paper along with the Vorticella's activities. This was done in order to ascertain whether these physical factors had any influence similar to their influence upon the tissues of higher animals. These tracings are omitted from the chart and from further consideration, because no hint of any connection or causal correspondence could be made out. As far as temperature is concerned, sudden change from ice water to water at room temperature (20-22 C.), or vice versa, never was observed to act as a stimulus sufficient to occasion stalk-contractions, nor did the rhythm of the contractile vesicle appear to be influenced in the slightest degree. This would seem to indicate that a vorticella is not endowed with even the rudiments of temperature sense. It is more difficult to disprove the influence of barometric pressure.

In all, fourteen experiments were made, similar to that just described, lasting from a few hours to five and a quarter days. Observation in all these experiments, especially the longer ones, was not continuous, though it was frequent during both day and night. The presence of several species of Vorticella and of Carchesium was utilized to add variety to the experiments. Farther than showing the physiological fact that the

rhythm of the contractile vesicle is somewhat different in the different species and tends to vary in a similar way under the same conditions, these experiments are simply confirmations of the first.

In Experiment 3, *V. gracilis*, observation was continued during the entire process of conjugation. Not the least change in the movements of the cilia, the taking in of food, etc., could be noted. Stalk contractions were frequent. During the hour in which the process was completed (9.42-10.48 A. M.) the vesicle-contractions gradually decreased in frequency from 8 to 2.6 per minute. Shortly after conjugation (11.09 A. M.) the stalk remained closely contracted and the bell detached itself and its movements could be no longer followed.

In later experiments by attending more carefully to food supply and by preventing as far as possible the growth of mould and bacteria and keeping the stream of water under the cover-slip as clean as possible, we were able to keep the *Vorticellæ* in apparently much more normal condition for a longer time. In spite of all efforts and precautions, however, mould and bacteria sooner or later overran the field and either killed the *Vorticellæ* or compelled them to migrate. We attempted to obviate this difficulty by sterilizing the water supply, and by boiling and covering antiseptically, at the same time giving in the place of their normal food a pure culture of yeast plants. This attempt resulted in an interesting demonstration of the educability of *Vorticellæ*. At first they took this, to them, newly discovered food with great avidity, filling their bodies to distention with food vacuoles of the yeast. In a very few minutes, however, the entire meal was ejected with volcanic energy. Not a single torula was allowed to remain in the body, and for several hours at least—how long the memory lasted was not determined—the individual could not be induced to repeat the experiment.

Experiment 11 was continued for two days and was terminated by accident. Experiment 14, upon *V. campanula*, lasted five and a quarter days and leaves no ground for doubting the truth of our main conclusion, viz., that a *Vorticella* works continuously and shows in its life no period of inactivity or rest corresponding to periods of rest in higher animals. In other words, a *Vorticella* never sleeps.

During the five days, frequent observation both day and night failed to detect any considerable relaxation of apparent effort or attention. The cilia worked uniformly, drawing in food particles and sorting them. In fact all efforts to surfeit the tiny animals with food produced no appreciable effect in satisfying their apparent hunger. Division occurred

frequently, but only in rare cases did this cause any noticeable relaxation of other work. Occasionally for a few moments during the act of division the bells became nearly spherical and their cilia worked feebly and in one instance ceased vibrating altogether. This had the appearance of a momentary "rest" but its occurrence was a rare exception to the rule of continuous work throughout the process of division. No instance of conjugation occurred in this experiment and this suggests a point of importance that has not as yet received attention. Under certain conditions a *Vorticella* passes into what is known as the encysted state, in which the bell becomes spherical, detaches itself from the stalk and secretes a cyst. It now is said to "rest" or "lie dormant" through a period of such unfavorable conditions as dryness or cold, and when circumstances favoring activity again return, it bursts its cyst, not as a single zoid, but as a number of small free swimming zoids. The stage of rest or encystment is thus also a period of reproduction. Each of the minute *Vorticellæ* attaches itself, develops a stalk and grows to the normal size of the species. Just the bearing which this phase of a *Vorticella*'s life has upon the problem of rythmical periods of rest and activity our experiments do not determine. So far as they yield any evidence, they support the view indicated above, which is generally adopted, that conditions unfavorable to life cause this mode of reproduction, conjugation and encystment. Encystment is a means of protecting the animal from changes in its environment which would otherwise prove fatal. Upon this supposition, as long as conditions of life remain favorable, a *Vorticella* might continue to live and work and reproduce by division indefinitely without the intervention of a "resting" stage. Encystment is therefore of the nature of an enforced "rest," a period of inactivity imposed by exceptional external circumstances; and therefore has no bearing upon the problem in hand.

During the course of this experiment, careful tests were made of the *Vorticella*'s sensitiveness to vibrations of sound and light. No one can watch a *Vorticella* for an hour without being struck by its exceedingly delicate sense of touch. The slightest jar is instantly answered by a quick contraction of the stalk, and particles scarcely visible under the microscope are sorted with the greatest apparent precision. No less striking throughout all the experiments was a *Vorticella*'s insensibility to all other stimuli. No reaction could be elicited to changes in light or to sounds of any kind so long as these were unaccompanied by perceptible jarring of the microscope. Musical sounds of all qualities and volumes were tried, but without effect. Bright sunlight was

flashed upon the creatures from total darkness; this was varied by interposing colored glasses, red, green, blue and violet, between the mirror and the stage of the microscope; each light was allowed to act for minutes at a time and was also tried in a succession of quick flashes, but not the least evidence of sensation could be detected. Thus, so far as we can judge, the universe must consist for a *Vorticella* of a series of touches, possibly also of tastes and smells; but not to any extent of sights and sounds.

— Correlating now what we have learned concerning the activities of this animal with the anatomical structures at its command, we remark first that a *Vorticella* consists for the most part of a mechanism for digesting food. Supplementing this is a motor mechanism beautifully adapted for securing it. Material thus obtained and assimilated causes the body to grow to a certain size, but when this limit is reached the body divides instead of enlarging indefinitely. A prime condition of the creature's life must be ability to distinguish food from what is not food. This it is able to do sufficiently by the sense of touch, and the ciliary mechanism which mediates this sense is precisely similar to tactile and sensory "hairs" as they exist throughout other parts of the animal kingdom. But like any other animal a *Vorticella* must be able to escape from its natural enemies.

The experiments afford evidence ample to prove that this is the chief purpose of the stalk and its contractions. Several earlier experiments were suddenly terminated by a "monster" appearing in the field and snapping off the *Vorticella*'s bell. In a number of cases contraction of its stalk actually pulled the bell out of a devourer's mouth. The particular enemy observed was a minute white worm hardly more than visible to the unaided eye. A necessary preliminary to later experiments consisted in carefully teasing all these animals out of the preparations. The sense by which a *Vorticella* is made aware of the approach of its enemy is touch. At least a *Vorticella* was never observed to react until its cilia were actually touched.

With food in abundance, capable of yielding a continuous supply of energy, it is a strange physiological paradox that all animals should not be able to work continuously. For any of the higher animals, at least, this is not possible. A certain amount of activity produces fatigue, and fatigue makes necessary a period of rest. Fatigue is commonly explained upon two assumptions. The first of these is that decomposition products arising from activity of the tissue are not removed quickly enough to avoid poisoning or clogging the organism. The second assumption is that highly organized ma-

terials are consumed during action more rapidly than they are formed. So generally are these processes present in the tissues of animals usually studied that we are apt to consider fatigue as a universal characteristic of living matter. It seems only reasonable, however, that protoplasm may be formed as fast as used under favorable conditions of nutrition, and that with equally good facilities for the removal of decomposition products, these may not accumulate in amounts sufficient to interfere with activity. So far as we are able to interpret the significance of our own experiments, this is the state of things in a Vorticella under favorable conditions of life.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY.¹

Made under the direction of
EDMUND C. SANFORD, PH. D.

VIII. A STUDY OF INDIVIDUAL PSYCHOLOGY.

BY CAROLINE MILES.²

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Great as have been the contributions of the laboratory to recent psychology, many most fascinating and important problems as yet resist experimental solution. For the study of these the investigator is thrown back upon introspection and observation, and, so far as his introspection is to have extraneous confirmation, upon the questionnaire.

To ask questions is easy, but to make the questionnaire an instrument of precision is very far from easy. It has more ways of going wrong than the chronoscope, and is in as great need of careful study. To say nothing of the general difficulty of selecting truly cardinal points for questioning about, and the special rhetorical difficulty of framing questions that shall be perfectly clear as to the information required without at the same time prejudicing the answers to be received, there yet remains the difficulty of assigning their proper weight to the answers received. How much dependence, for example, can be placed on the inner observations of people of intelligence, but untrained in introspection? How fully does what can be recalled at the time of answering represent the total experience of the answerer on the matter in question? What allowance must be made for influences that might unconsciously mold the answers, — aversion to displaying anything of the inner life on one hand and egotistic interest in one's own experiences on the other, distrust of the questioner or desire to

¹ Continued from Vol. V, p. 389.

² Received as a private pupil during the winter of 1893-94.

stand well in his estimation? Some of these sources of error can be avoided, some must be recognized and allowed for and some must forbid the use of the method except under uncommon circumstances. On some of these points the questionnaires already put forth by various investigators have thrown light, and the following report of a study by this method is presented as much for what it may contribute to the psychology of the method as for the facts that were elicited by it.

The questions asked were as follows :

- A. (1) How do you know your right hand from your left? (2) Do you ever hesitate for a moment as to which it is? (3) Are there any two things that you persistently tend to confuse, such as *ei* and *ie* in spelling, or the Guelphs and Ghibellines in history? (4) Have you mastered any such tendencies and how?
- B. How do you recall a forgotten name?
- C. (1) How do you concentrate your mind with all your might on some one thing, *e. g.*, in playing the parlor game of "mesmerism"?¹ (2) How do you force yourself to work when you do not want to? (3) How do you pay attention to a dull lecture?
- D. How do you go to sleep when sleepless?
- E. (1) What things were you afraid of as a child? (2) Were you ever frightened by these things? (3) How did you overcome your fear?
- F. Mention a good ghost story, *i. e.*, something that gives you the creepy feeling supposed to characterize ghost stories
- G. Mention several concrete instances of things that have made you angry—ten if possible.
- H. (1) What is your favorite color, *i. e.*, what color appeals to you most apart from any colored thing—merely as color sensation? (2) Why do you like this color? (3) Has this color any association with persons, places, music, poetry, emotion, odor, taste?
- I. Did you express yourself in any art form before eighteen years of age?
- J. What were your favorite games when a child?
- K. (1) What is the earliest thing you are sure you can remember? (2) How old were you?
- L. Mention some story that has made you weep—the most pathetic you can think of.
- M. Mention a funny story, incident, joke or scene in a book or play—the funniest you know, if possible.
- N. What characters in history, fiction or life were ideal to you when growing up?
- O. If you had just one sermon to preach what would be your text?

These questions were asked of 100 Wellesley women during the winter of 1893-94. The persons were taken at

¹ This game has several forms and many names. The essential feature of it, however, is that one member of the company withdraws while the remainder select some object which he is to find or some act which he is to perform on his return. When he re-enters all of the company endeavor to assist him by intent concentration of mind on the object or act selected.

random and the number included seventy-one students and twenty-nine members of the faculty. At first the questioning was verbal and the entirely naïve answers were noted down by the writer. These results were then examined and two months later other questions suggested by them were added to the original set and a printed copy was sent to the same 100 persons with the request that new answers be written without regard to the former answers. Ninety-seven persons responded, and the following report is based on a tabulation of these written answers. Comparison with the first naïve answers has also been made when it promised anything of interest.

The questions were asked in irregular order, but will be discussed in five rough groups: I. Habits of Discrimination and Memory (*A* and *B*). II. Method of Concentrating Attention and of Getting to Sleep (Extreme Distraction of Attention) (*C* and *D*). III. Emotions and Preferences (*E*, *F*, *G*, *H*, *I*); *E* strictly belongs to group IV, but for purposes of comparison with *F* it has been placed as it stands. IV. Recollections of Childhood (*J* and *K*). V. Miscellaneous Questions More or Less Unsatisfactory (*L*, *M*, *N* and *O*).

I.

Habits of Discrimination and Memory.

A. (1) Question: How do you know your right hand from your left?¹

Replies on second questioning (97 cases): Thirty-three (33) mentioned some association, such as writing or eating, the position of the heart, some actual difference in the hands, as a ring or scar, or the conditions in which the respondent was when she first learned her right hand. Twenty-seven (27) replied that there is a distinct difference in feeling, which they describe as readiness, skill or strength. Thirty-seven (37) called their method merely "instinct," and could not define the difference further.

Replies on first questioning (100 cases): Thirty-seven (37) mentioned an association, thirteen of them mentioning some actual difference in the hands. Thirty-three (33) said it was a feeling of difference, three stating that the difference extended to the whole side. Thirty (30) called it "instinct," or replied "because I was told," without further specification. The greatest change in the records of the two questionings is the decrease of those that tell by a feeling of difference and the increase of those that tell by "instinct."

¹ For this question the writer is indebted to Dr. Clarence Blake of Boston.

Such a change is not surprising considering the indefinite character of the feelings of difference reported, a difference in readiness is hardly to be distinguished from an instinctive difference in the loose way in which the word was used. The three persons that fail to appear in the second questioning belonged one to each class.

(2) Question: Do you ever hesitate for a moment as to which it is?

Replies on second questioning (97 cases): Fifty-seven (57) replied Never. Forty (40) replied Yes. This is probably too small a proportion for those liable to this confusion. Of the forty that replied Yes, nearly a quarter (9 — all students) gave such instances as drill and setting tables, as in Wellesley domestic work, but not all of the students questioned (71) had had drill or setting tables, and some therefore had missed these opportunities of finding their liability to confusion.

A comparison of the answers to (1) and (2) shows an interesting relation between the way of telling the hands and liability to confusion.

Of 33 who tell by association:	24 are sometimes confused.
Of 27 who tell by a feeling of difference:	10 are sometimes confused.
Of 37 who tell by "instinct:"	6 are sometimes confused.

This relation furnishes another evidence of the greater reliability of muscular as compared with associative memory, and of unconscious as compared with conscious memory of either sort. The difference, however, is probably a good deal less than the figures seem to show, for the persons that suffer confusion and so have to recall consciously which their right hands are would be on that account the more able to give a definite account of how they know, when presented with a questionnaire. Both tendencies work in the same direction: those that are uncertain are familiar with how they find out, and those that find out by conscious means are apt to be uncertain.

Of the 97 persons asked, 43 remember how they learned and 54 do not. Twelve now tell the hands by recalling the way in which they learned; eleven of these belong to the forty-three who remember and one, whose knowledge came in some other way, to those who do not remember.

The fact that the method of learning still persists as an association may be due to a very good memory or it may be due to learning very late. Nine of those who distinguish by an association mention learning after they started to school. The average age of the earliest memories (see question K below) of the two classes seems, however, to favor the former hypothesis. The age to which the earliest memories of those

who employ association belongs averages three years; that of those who know by instinct averages three years nine months; that of those who know by difference in feeling averages three years seven months.

The tendency to confuse the right and left hands called attention also to the tendency to confuse other pairs of things that have few points of difference or in which from any reason the distinction seems arbitrary.

(3) Question: Are there any two things which you persistently tend to confuse, such as *ei* and *ie* in spelling, or the Guelphs and Ghibellines in history?

Replies (97 cases): Seventy-five (75) were conscious of more or less tendency to confusion, some saying that they confused everything that could be confused. Twenty-two (22) were not conscious of any such tendency. The list of examples given by the seventy-five may be classified as follows, beginning with the most arbitrary distinctions: (a) *Spelling* and other confusions almost as arbitrary: e. g., directions, turning a screw. Spelling, however, afforded most examples, such as double letters, *el* or *le*, *sion* or *tion*, etc. (b) *Words of similar sound but different meaning*: e. g., statue and statute, Calvary and cavalry. (c) *Mathematical formulæ* which must be used in computations as words are used in sentences. These involve a train of reasoning too long to go through each time they are used, and are, therefore, distinguished arbitrarily in memory: e. g., *sin* ($x+y$) and *sin* ($x-y$). (d) *Confusions in science*: e. g., do acids or alkalis turn litmus paper blue? Of two organ pipes of the same length, which gives the higher note, the open or the closed pipe? These involve a process of reasoning, but it is not so long as the mathematical formulæ and is more concrete, so the memory can be aided by associations with experiments and by mental images. (e) *Confusions in history*: e. g., did the Yorkists or the Lancastrians wear the red rose? Did Roger or Francis Bacon write the "*Novum Organum*?" With increasing knowledge of history these facts cease to be mere matters of memory and it becomes impossible to confuse them. Taking into account also the fact that the number of confusions decreases steadily from the most arbitrary to the least so, from spelling which has almost no necessary associations, to historical names which abound in them, the conclusion is obvious that the tendency to confusion varies inversely as the fullness and variety of the associations that are started by the ideas in question.

This list of things confused was further examined to find the nature of the confusions made by those who distinguish their hands in the three typical ways.

Of 33 who distinguish by association:

28 were liable to confusions of the sorts mentioned.

10 gave instances in classes (a) and (b).

18 gave instances in classes (c), (d) and (e).

Of 27 who distinguish by feeling:

13 were sometimes confused.

9 in spelling.

4 in other cases.

Of 37 who distinguish by instinct:

24 were liable to such confusions.

21 in spelling, class (a).

14 in classes not purely arbitrary, (c), (d) and (e).

The first and third of these groups seem to show that the instinctive method of deciding is less valuable in the later acquired forms of discrimination. This agrees with the general biological principle that instinctive action proves valuable in relatively simple conditions to which the organism is well adapted, but must be replaced by conscious action when the conditions become complex and adaptation less perfect. Most of those who have no confusions are among the class who distinguish their hands by a difference in feeling, who, perhaps, carry spelling and formulæ and even historical associations in motor terms.

(4) Question: Have you mastered any such tendencies, and how?

Of 68 who answered this question:

29 employ an arbitrary association.

15 employ reasoning, *i. e.*, they think of the meaning of two terms or the derivation.

6 use arbitrary memory—some call it sheer force of will, others strain of attention, with some it becomes positive muscular strain.

18 have not succeeded in mastering their confusions.

A comparison of the records of the first and second questionings for questions (3) and (4) shows that fifteen persons find new confusions and seven omit old ones; and that nineteen seem to have changed their method of mastery, eight find arbitrary memory unsuccessful, seven find confusion not overcome, four are scattering—rule instead of arbitrary memory and *vice versa*.

B. Question: How do you recall a forgotten name?

Replies on second questioning (97 cases): Eighty-eight (88) recall by some sort of association. Two (2) are conscious of a strain toward vacancy. One (1) calls up a mental image of the name [tries to visualize it?]. One (1) is unsuccessful by any method, and five (5) cannot tell how they recall a name. A further classification of those that work by an associative clue, gives the following results:

Of 88 making use of some sort of association:

51 seek by association with the person to whom the name belongs, the circumstances under which it was first heard and the like.

24 by the initial letter, or the place on the roll.

12 by the sound of the name.

1 by some peculiarity of the spelling.

It seems probable that the common habit in trying for a lost name is first to recall the image of the person whose name it is, or the circumstances in which the name has been heard, or some other complex image in which the name is a part. This may happen almost unconsciously. If this method fails, appeal is made to some one of the more conscious methods, in which attention is directed to some remembered part or mark of the name, in the hope that this will bring up the rest. The fifty-one above make use of the first method, going from whole to part; the remaining thirty-seven use the more artificial method and go from part to whole.¹ The initial letter is apt to be the part recalled both because of its prominence as a capital letter and because of its being the first in the series that make up the name.²

The chief difference between the records on this point, in the first and second questionings, is the decided gain in the group that makes use of the first method. In the first questioning the groups were nearly equal; in the second, a small class who had depended upon some peculiarity of the leading consonant or vowel, length, rhythm, color or a vague indefinable feeling of recognition, almost entirely disappeared into the class that recall by association. This change may very likely have been due to better observation on the part of the answerers, induced by the attention which the first questioning called to the matter. It may also have been due to a disinclination to specify particularly a second time.

II.

Methods of Concentrating Attention and of Getting to Sleep.

C. (1) Question: How do you concentrate your mind with all your might on some one thing, *e. g.*, in playing the parlor game of "mesmerism?"

The naïve answers on this point in the first questioning were of three types:

¹It has already been found in association experiments that transition from whole to part is more frequent and probably easier than transition in the contrary direction. (Cattell and Bryant, *Mind*, Vol. XIV, 1889, p. 241.)

²Experiments on memory span have showed the superior persistence of the first member of the series used: *cf.* Bolton, this JOURNAL, Vol. IV, p. 378 f.

(a) Those who were conscious of some physical strain which aided in concentration, *i. e.*, a feeling of exerting will-power in mentally repeating the command "Do so-and-so;" or a tension of the vocal organs in repeating over and over the names of the single object which the "mesmerized" person is desired to find; or a tension in the head, or an effort to keep the body quiet.

(b) Those who are more conscious of a mental image of the object or of the act to be performed. A few see the object in all its parts or relations, and one sees the word.

(c) A very small class are conscious of auditory sensations, *i. e.*, they hear the word, either with or without repeating it.

A few others can give no idea how they do concentrate.

The written answers of the second questioning confirm this result, but the first class is much larger than the others. Repetition seemed clearly a means of concentration. The third class almost entirely disappears, and the number of those who cannot tell increases.

(2) Question: How do you force yourself to work when you do not want to?

Replies on first questioning (100 cases): Sixty-one (61) think of the end to be attained, duty to self or to some other person, the necessity of getting done promptly, consequences of doing or leaving undone. Twenty-four (24) mentioned some physical device, *e. g.*, sitting up straight, reading aloud, reading over and then stopping to repeat, or some bodily comfort conducive to "cramming." Fifteen (15) could say only "I just go to work."

The sixty-one give the usual testimony as to voluntary attention; in forced work the present physical discomfort is hidden by the pictured future happiness or unhappiness. It also appears that, when duty and inclination conflict, the fear of evil consequences is a more powerful, or at least a more frequent, motive than the hope of good to be attained.

Replies on second questioning (97 cases): Forty-five (45) mention duty, necessity, fear of consequences, three thinking of the object of the work and one of the reward. Twenty (20) speak of physical tension. Eighteen (18) have some special method of going to work, and fourteen (14) cannot tell.

Among the methods mentioned are: Close attention to details, removal of all external distractions, reading aloud, timing one's self, not allowing one's eyes or hands to wander, feigning an intense interest, and imagining one's self another person who is not tired or who wants to work. One speaks of artificial stimulation with coffee.

(3) Question: How do you pay attention to a dull lecture?

Results of second questioning (96 cases): Sixty-four (64) have some conscious method. Ten (10) think of losing an opportunity, of politeness, etc., *i. e.*, make listening a matter of conscience. Five (5) feel physical strain. One (1) never finds a dull lecture, and sixteen (16) do not listen if they find one.

The answers to this question bring out another phase of attention. Not physical strain, nor moral consciousness is prominent, but the means of getting hold of the subject-matter that is presented. The number of those who do not listen includes ten members of the faculty who are, of course, beyond the stage of compulsory attention to dull lectures and sermons. The smallness of the number of students remaining is doubtless due to the fact that the question was explained to mean: How do you pay attention to something you are to be examined upon hereafter?

A classification of the methods employed by the sixty-four who have conscious methods gives the following table:

Of the 64 that have some conscious method:

- 16 pay close attention to the words, repeating them after the speaker if necessary.
- 14 feign an interest. This includes taking the attitude of interest.
- 12 make an outline. Some mentally, some on paper.
- 11 look steadily at the speaker.
- 6 assume a critical attitude.
- 4 try to get into rapport with the speaker.
- 1 imagines each sentence is the last.

Two things are strongly marked in this table. The first is the appeal from the sensory and receptive functions to the active and more directly controllable motor functions, from simple hearing to repeating of the speaker's words, the taking of an attitude of interest, the formation of an outline. The second is the turning to details, to the relatively concrete, either in the sensory form of the individual words or sentences of the speaker or his person (groups of 16, of 11, and of 1) or in the more intellectual forms of the outline and the critical attitude. The fact, also, that attention can be encouraged by feigning interest is worth regarding, not only for its implications as to the nature of emotions in general, but also as a contribution to mental tactics. All of the methods, however, probably involve more or less fully both the mental and the physical attitudes of attention.

A comparison of the answers to questions (1), (2) and (3), received at the first questioning with the later written answers, shows in (1) more consciousness of physical strain,

and in (3) more discovery of method. In (2) there is little difference, the changes balancing each other.

D. Methods of Getting to Sleep.

Question: How do you go to sleep when sleepless?

Replies on first questioning (100 cases): Seventy-seven (77) report some method, twenty-three (23) report none (twelve because they are never sleepless and eleven because when they are sleepless no method succeeds).

Of 77 who report methods, 9 mention a physical method;

68 are mental and are as follows:

- 19 try counting.
- 10 repeat poetry.
- 8 have various methods of thinking of nothing.
- 6 feign the state of sleep.
- 4 make up stories.
- 4 imagine that they are rocking on the ocean.
- 4 try to think of something pleasant.
- 3 imagine sheep going over a stile.
- 2 make pictures of peaceful or monotonous scenes.
- 2 try to think of something dry [uninteresting].
- 2 confine attention to some one thing.
- 1 listens in imagination to a brook she has often heard when going to sleep.
- 1 listens to the ticking of her watch.
- 1 repeats the Greek verbs.
- 1 counts her breaths.

Many had more than one method, and many mentioned both physical and mental methods.¹

In the first questioning there was also no report as to whether the answerers were troubled by sleeplessness or not. The printed questions asked for physical and mental methods and added: Are you often troubled? The replies (97 cases) showed twenty-two (22) who were often sleepless, nineteen (19) who were rarely so, fifty-six (56) who were never so. A further classification of the methods employed by these different groups gives the following table:

Of 22 who are often sleepless:

- 6 find a physical method more successful, *e. g.*, more fresh air in the room, eating, lying on the chest, perfect relaxation, etc.
- 4 must get up to read awhile.
- 3 count.
- 3 repeat something.
- 1 imagines humming sounds.

¹Compare the list which Wordsworth gives as ineffectual in his sonnet on "Sleeplessness."

"A flock of sheep that leisurely pass by
One after one; the sound of rain and bees
Murmuring; the fall of rivers, winds and seas.
Smooth fields, white sheets of water, and pure sky:
I've thought of all by turns, and still I lie
Sleepless:"

- 1 imagines an agreeable company.
- 4 are not successful by any method.

Of 19 who are rarely sleepless:

- 4 try counting.
- 2 have methods that suggest hypnotism, following an imaginary line up and down or watching a mental image of interweaving lines and concentric circles.
- 1 reads.
- 1 repeats something.
- 1 performs a simple arithmetical problem.
- 1 is unsuccessful.

The other 9 try stories, air castles, imagining the influence of ether, or merely anything pleasant, only one mentioning a real physical method; the others seeming to be merely memories of how it is to go to sleep.

Of 56 who are never sleepless:

- 9 think of nothing.
- 7 mention physical relaxation.
- 7 count.
- 7 think of something pleasant.
- 3 repeat something.
- 10 have no conscious method.

The other 13 have methods which are special instances of thinking of something pleasant, but involve regularity of movement, or simple mental imagery.

The writer's conjecture, formed after examining the first collection of methods, that the best of them is the feigning of sleep (chiefly a physical method) was confirmed by the relative frequency of physical methods among those that are really sleepless. Counting, repeating poetry or Greek verbs, rocking on the ocean, listening to a watch or to the imaginary sound of a brook, while they are rhythmical and would go with regular breathing, seem to imply a little more attentive activity for their execution. Those who make stories or pictures, or think of something dry, or concentrate attention on some one thing, seem to feign dreaming or to remember the semi-conscious reverie that precedes falling asleep. This also is confirmed by the classification, for such methods are tried by those rarely or never sleepless. Both these methods and the feigning of sleep proceed on the psychological principle that a given mental state will tend to reproduce itself entire when enough of its constituent parts are reproduced.

Of the psychical causes of sleeplessness the chief is inability to cease thinking about what has engaged the attention before going to bed; it is a "cramp of attention." The great art in getting to sleep on the other hand is the art of attending to nothing, the art of general distraction of attention. This makes the methods employed in getting to sleep an interesting counter picture of the methods of concentrating attention in study or on a dull lecture. In the first there is general

passivity, in the second activity. To attend, the muscles are contracted and an erect posture is assumed, explicit movements are made (reading aloud and repeating what has been gone over, making notes of what is said by the lecturer); to get to sleep, the muscles are relaxed, a recumbent position is taken, movements are avoided or suppressed (the counting or reciting poetry is generally silent or semi-articulate). In attending, the effort is to establish trains of thought connected with the matter in hand (making outlines, taking a critical attitude, looking steadily at the speaker, and trying to put oneself into rapport with him); in getting to sleep, the effort is to establish trains unconnected with the intrusive thought (reading, counting, reverie, repeating poetry or verbs). Over against the tendency to concentration on details in the first may be set in the second the tendency to rhythm (which gathers particulars into groups), though rhythm has a yet stronger reason for its presence in that it reduces the demands on attention to a minimum. In the first, future consequences are a spur, in the second everything but the present is excluded; and further instances would be easy to find. The contrast, however, is not absolutely perfect, and there are even points of identity, because, while attention is concentration as opposed to general distraction, it is itself distraction from everything except the thing attended to. In attending there is the exclusion, so far as possible, of sensations unconnected with the matter in hand, in getting to sleep the exclusion of all sensations; in attending the more or less complete suppression of non-assistant movements, in getting to sleep the suppression of all movements. Both are sought by imitation of the characteristic attitudes of mind and body.

A comparison of the results of the first and second questionings as wholes shows that in the two months that intervened physical methods had in six instances proved more successful than mental; in four instances building air castles, imagining something pleasant, etc., had proved better than no method; in four other instances reading, physical relaxation, etc., had proved better than counting; two persons had found no method successful.

III.

Emotions and Preferences.

E. (1) Question: What things were you afraid of as a child?

The answers received on the second questioning are classified in the following table. The number of things feared

exceeds the number of persons questioned, because most persons who confessed to any fear confessed to more than one.

Classification of things feared:

- 31 feared darkness.
- 31 feared animals. Dogs and cows were mentioned most often, geese and turkey gobblers several times.
- 24 feared a class of creatures which many of them said caused repulsion rather than fear, *e. g.*, snakes, spiders, worms, mice, cats, etc. Many in this class seemed abnormally fearful, and were terrified, also, by floating feathers, tearing cloth, and by all white, fuzzy things.
- 18 feared human beings—drunken, dead, insane, strange tramps, and rude boys.
- 9 feared imaginary evils, *e. g.*, witches, Satan, the end of the world, being buried alive, earthquakes, nightmare.
- 3 spoke of thunderstorms or of all strange noises.
- 2 were afraid of everything, one of whom was sure it was due to a prenatal influence.
- 6 were afraid of nothing.

Taking the list as a whole it is easy to trace in it two types of fear, the artificial fears induced by painful experience or the suggestions of elders, and the instinctive fear of the unknown. From the latter type, perhaps, ought to be separated yet a third, namely, fears that arise reflexly on sense impressions of special kinds, especially voluminous sensations. Examples of this would be the fears of snakes, spiders and insects and of fuzzy things, so far as these are tactual, the fears of tearing cloth, of thunder and other loud noises, and perhaps in some cases the fear of darkness—probably the most voluminous visual sensation in the experience of a child.¹ These are, however, at first all more or less strange experiences and later are seized upon by suggestion, so that in any individual case classification would be difficult, if not impossible. In the more conscious fears there is a common

¹The following extract from the autobiography of Laura Bridgman seems to show something like an instinctive touch-fear, though, of course, strangeness was also a factor. The experience belongs to a time before her eighth year, and so, before means of communicating with her had been secured. "My father used to enter his kitchen bringing some killed animals in, and deposited them on one of the sides of the room many times. As I perceived it, it made me shudder with terror because I did not know what the matter was. I hated to approach the dead. One morning I went to take a short walk with my mother. I went into a snug house for some time. They took me into a room where there was a coffin. I put my hand in the coffin and felt something so queer; it frightened me unpleasantly. I found something dead wrapped in a silk handkerchief so carefully. It must have been a body that had had vitality. I did not like to venture to examine the body for I was confounded. There stood some person on one side of the floor very calm, gazing upon the dead, and they touched its clouded eye and stroked it as if the tears were shedding along his face."

element of helplessness, in the dark one is robbed of his chief sense; ghosts, witches, Satan, represent an unknown and invisible power; earthquakes and the end of the world are catastrophes against which no power is availing.

The answers received on this head in the first questioning were unreliable, because the respondents did not at once recall the things they feared most, and those of the second questioning have alone been regarded in this discussion.

(2) Question: Were you ever frightened by these things?

This question might be expected to decide in individual cases whether the fear was acquired by personal experience on the one hand, or by instinct or suggestion on the other. The answers to this question were inconclusive, about as many fears of each kind appearing in the lists of those who had not been actually frightened by them as in the lists of those who had.

(3) Question: How have you overcome your fear?

Most of those who had not actually been frightened spoke of the fear as outgrown. A large class still feared snakes, insects, cows, horses, drunken and insane people. A few said they reasoned themselves out of it; one said pride overcame it, another, that better health caused it to disappear naturally. Much the greater number of fears caused by actual fright had not been overcome; repulsive things formed the largest class of these. Twice as many childish fears persist when caused by fright as when no cause can be remembered and they are believed to be instinctive.

F. Question: Mention a good ghost story, *i. e.*, something that gives you the creepy feeling supposed to characterize ghost stories.

Replies on second questioning (97 cases): Eighty (80) told stories which they thought creepy. Twelve (12) could tell none, were not sufficiently impressed to remember them. Five (5) told stories with a humorous turn at the end, which seemed to be the thing for which they were remembered, not for the mysterious part. The stories mentioned were all read to find the element which gives the creepy feeling. Many, of course, involve several elements of the fearful, but such a classification of elements as can be made gives the following table:

Of 80 stories which were thought creepy:

32 involved something unexplained, *i. e.*, one-third of all the replies were real ghost stories.

22 were stories of insanity.

16 were stories involving moral horror, as well as other sorts of fear, *e. g.*, Dr. Jekyll and Mr. Hyde.

10 were stories of murder, torture, snake stories, or stories about finding a corpse unexpectedly.

While none of the categories under *F* exactly match those of *E* and while *F* asked for stories of one kind of fear only, some general points of resemblance are interesting. The stories of murder, torture, snakes, and the like in *F*, are analogous to the sense fears in *E* and perhaps depend more immediately on sense imagery for their effect than the others. Stories of insanity and the fear of the insane are of common origin. The real ghost stories involve darkness and its fears together with those of the unknown and of mysterious power. An entirely new kind of fear appears in the moral horror group, a mark perhaps of the adult audience for which such stories are written. The feeling inspired by "Dr. Jekyll and Mr. Hyde," or by Mrs. Shelley's "Frankenstein," swallows up the mere physical sensations and makes them instruments of a moral repulsion. Hyde's external appearance and his crimes are repulsive to contemplate, but the story means little to him who sees only the bare incredible facts. Ibsen's "Ghosts" is repulsive as any idiocy is repulsive, but there is more than mere idiocy, there is awe before the forces of nature which make sin its own punishment. This is like the fear of darkness in that it is individual helplessness, but it is much more complex. It includes many kinds of sense-fears plus associations with moral ideals that do not exist for the young child. The ghost stories that affect one most are those in which there is a skillful accumulation and interweaving of all sense-fears. Among these ghost stories Poe's tales, Lytton's "The House and the Brain," and a story, originally from the German, called "The Gold Arm," were mentioned equally often. Stories of being watched by a pair of eyes peeping through a rent in a curtain or a crack in the floor received the next highest number of votes, and after them a story in *Harper's Magazine* for 1859, called "What was It?" An examination of the plots of these stories shows most interestingly how the artificial fear is worked up; fears of the sense types are common but generally subordinate, the fears of others are described and excite our own by sympathy or imitation, the whole scene of the story is gradually shifted from the ordinary world of daylight and known forces to a world in which man is the sport of mysterious and unknown powers. The appeal to the senses is never to all at once; a presence can sometimes be felt but not seen, sometimes seen or heard but not touched; sometimes it is only the effects of its acts which appear. The actual shudder of fear is generally the result of a special sensory appeal.

G. Question: Mention several concrete instances of things that have made you angry—ten if possible.

In order to secure a large number of answers no time of life was mentioned and thus the instances extend from childhood to the age of the average Junior in college. Those who wished to respond kept asking: "Do you mean vexed, or indignant, or simply *mad*?" The latter definition was adopted because it seemed least likely to be misunderstood. Of 100 persons asked only 34 gave lists of things that had made them "mad."¹ These lists included 247 instances of anger, but thirty-two instances were duplicated in the same list, *i. e.*, the same person unconsciously gave two or more instances when the cause was the same in both. The instances duplicated were as follows: Seven gave more than one instance of being angered by punishment; four by punishment because it was thought unjust; six by reflections (slurs) upon family, friends, country, etc.; five by arbitrary compulsion; two by pride in personal appearance; two by meddling; two by inanimate things; one by false accusation; one by carrying off of property. A classification of the 215 single instances remaining gives the following table:

Of 215 single instances of anger:

78 were due to causes which the writer cannot better name than injuries to sense of personal dignity; seventeen, remarks more or less insulting to members of one's family, dear friends, sex, church, or political party; sixteen, punishment, including scolding, punishment that was administered in the presence of others, etc.; seven, interference in one's personal affairs, *e. g.*, religious beliefs or friendships; six, being laughed at; four, being slighted; four, making a bad appearance in recitation; three, not being told what one thought she ought to know; two, being patronized; two, being gossiped about; two, being made a tool of; two, having manners corrected; two, comments on personal appearance; the rest of the seventy-eight are isolated instances, such as being told, "I told you so," having work taken out, etc.

73 are caused by sense of injustice;

41, injustice to self: twelve, unjust punishments; eight, when someone shirks her duty; seven, being contradicted—including positive accusation of falsehood; five, a broken promise; two, having to spend more time on lessons than seems just; one, being willfully misrepresented; one, being interrupted; one, not being allowed to explain; one,

¹ The smallness of this proportion is doubtless due in part to the difficulty of recalling things that have caused anger (normal people put such unpleasant things out of mind and in time forget them) and in part to disinclination to confess those that are recalled. The question is emphatically condemned as a question by this small number of answers and except for the goodly number of instances cited by the third that did respond would have hardly been worth discussing.

- being cheated out of something; one, being left at home; one, not being sympathized with; one, when matters serious to her were made light of.
- 17, injustice where self is not involved: eight, seeing someone else punished unjustly; four, hearing some one unjustly criticized; three, seeing animals ill-treated; one, seeing good nature imposed upon; one, seeing one person tell on another.
- 15, injury to property: eight, interference or injury to property, *e. g.*, having a notebook carried away; five, when a journal or letter was read; two, when a pet was killed.
- 33 were arbitrary compulsion, *i. e.*, to do one thing when she preferred to do another, or to refrain from something, or simply to "do as you are bid," *e. g.*, to do some kind of domestic work, to practice music, to apologize, not being allowed freedom to do work in one's own way.
- 25, physical annoyances: ten, being teased; three, "the mere sight of some people;" two, hurting one's self; two, familiarity from some people; two, having one's hair combed. Other examples are: pianos out of tune, being pushed in a crowd, being kept awake at night, etc.
- 6, disappointment, *e. g.*, not being able to learn something, or to find something when it is wanted, or missing a train.

Such a table as this, based on answers from only about one-third of those asked, can furnish only the most general indications, but there are traces of a few relations that are, perhaps not entirely accidental. The preponderance of mental and moral causes is clear and to be expected. The extent to which offences against personal dignity appear (and to the seventy-eight specified in the table should surely be added a large portion of the cases of injustice to self and of arbitrary compulsion) is doubtless characteristic of the angry emotions of people generally, and is not without pedagogical and ethical import. The small number of cases in which the anger was altruistic, testifies to a healthy egoism; we may be indignant at injuries to others, but our feeling rarely rises to anger unless others stand near enough to us to be covered by the *égoïsme à deux* by which a certain Frenchman has described love.

H. (1) Question: What is your favorite color, *i. e.*, what color appeals to you most, apart from any colored thing—merely as color sensation?

Replies on first questioning (100 cases): Thirty-eight (38) prefer some kind of blue; eighteen (18) some kind of red; twelve (12) yellow; eight (8) green; five (5) violet; one (1) white; one (1) dark brown; one (1) all dark warm colors; one (1) mere brightness of evening sky; and fifteen (15) have no preference.

Replies on second questioning (97 cases): Thirty-seven (37) preferred blue; twenty-two (22) red; ten (10) yellow;

nine (9) green; five (5) violet (heliotrope, lavender, purple); two (2) brown; two (2) gray; one (1) white; one (1) brightness of the evening sky; one (1) all dark warm colors; and seven (7) had no preference.

Blue is clearly the most generally preferred color and red stands next; after it follow yellow, green and violet. The most noticeable difference between the results of the two questionings is the decrease of those who have no preference and the gain of those that prefer red.

This order is confirmed by the report from a psychology class, each member of which was asked to write her favorite color on a slip of paper before leaving the room. This class of twenty-one reported as follows: Ten blue, four red, two none, one yellow, one gray, one violet, one green, one white.

Another class of forty-six was asked suddenly to put down the first color that came into their minds. Of forty-six persons: Nineteen wrote red, fifteen wrote blue, five wrote yellow, four wrote white, one wrote green, one saw the whole spectrum beginning with red, one saw a band of four colors beginning with red.

The explanation of color preferences was sought by asking the second question.

(2) Question: Why do you like this color?

In the first questioning:

Of 38 (blue): 30 give a reason.

Of 18 (red): 16 give a reason.

Of 12 (yellow): 10 give a reason.

Of the smaller classes all can tell why, and the reasons are most elaborate for the most unusual colors.

In the second questioning:

Of 37 (blue): 29 can give some reason.

Of 22 (red): 19 can give some reason.

Of 10 (yellow): 8 can give some reason.

Of 9 (green): 8 can give some reason.

All after green as before.

From this it appears that in both cases red has more conscious meaning than blue or yellow. The reasons given for liking blue are, delicacy, purity, tenderness, spirituality, infinity, calmness, faithfulness, immortality. Red is chosen because it is warm, deep, cheerful, loving, intense, passionate or quivering with pulsating life. Reasons vary in both cases with the shade chosen. Yellow is chosen for warmth, softness, happiness.

(3) Question: Has this color any associations with persons, places, music, poetry, emotion, odor, taste?

In the first questioning:

Of 38 (blue): 26 have some association.
 Of 18 (red): 10 have some association.
 Of 12 (yellow): 9 have some association.
 All after yellow have some association.

In the second questioning :

Of 37 (blue): 26 have some association.
 Of 22 (red): 13 have some association.
 Of 10 (yellow): 6 have some association.
 Of 9 (green): 7 have some association.

Blue is most associated with the sky, then with memories of childhood, with the sea, with music, and all the gentler feelings. Red is associated with strong feelings, strong characters, autumn, martial music. Both are frequently associated with persons. Yellow is associated with flowers and sunshine.

The conscious reasons are relatively more numerous in the case of red than in that of blue, but the explicite associations are less so. This would seem to mean, if the figures will bear any interpretation, that red is a color of somewhat more direct emotional meaning, and blue of somewhat more indirect meaning. The "direct meaning" of any color is probably dependent on early and forgotten association, and the indirect on later and better remembered association. If this is the case the direct emotional meaning of red would probably be a trace of the supposed preference of very little children for that color. How much the answers are influenced by such expressions as "true blue" would be hard to say, but there is not much trace of it in the "reasons" and "associations" specified. Early habits of dress may count for a good deal (more children seem to be dressed in blue than in red), but this still leads as a problem why mothers choose blue.

I. Question : Did you express yourself in any art form before eighteen years of age?

Replies on second questioning (97 cases) : Sixty-six (66) Yes ; thirty-one (31) No. Two (2) used some form or forms, but do not say which. A classification of the forms used by the sixty-four remaining gives the following table :

Of 64 who made use of some art form:

14 used verse (alone).
 10 used stories (alone).
 6 used drawing or painting (alone).
 4 used music (alone).
 14 used stories and poetry.
 3 used stories and drawing or painting.
 3 used poetry and drawing or painting.
 2 used poetry and music.
 1 used painting and music.
 6, all arts but music.
 1, all arts but painting.

Those who replied No seemed to take pride in the fact that they had been guilty of no such youthful folly. Most called their poetry rhymes. No attempt was made to get possession of the productions, but information was volunteered in some cases. Most interesting was one who wrote a tragedy at ten, which was acted on a little stage for the benefit of her friends; from ten to thirteen, an epic; at thirteen, sentimental and religious poems. A few mentioned telling stories as a favorite pastime, but said they did not write them down. A few mentioned a full journal as one means of expressing feelings from about twelve to fifteen. A few more who were asked in general conversation, said their poetry was religious and their stories sentimental—often involving their idealized selves as heroines.

IV.

Recollections of Childhood.

J. Question: What were your favorite games when a child?

The list of games falls easily into active, imitative and competitive groups. Classifying in that way gives the following table:

Of 92 who replied:

49 gave active games (including those of competitive activity):

31, running and being pursued; specifically: sixteen, hide and seek; six, all running games (none named); four, tag; chase, London bridge, pussy wants a corner, blind-man's buff, fox and geese, hop-scotch, mulberry bush, wild Indians, etc.

13, mere activity (?): horse, all boys' games, all out-door games, climbing trees, riding horseback, swinging, etc.

5, games involving more thought: four, ball; one, hunt the thimble.

36, imitative games: thirteen, dolls; twelve, dramatic plays (invented from stories); four, keeping house; four, school; two, building bridges; one, church.

7, competitive games (involving thought): three, checkers; two, dominoes; two, puzzles.

Evidently far the larger number preferred out-door games, and among these most prefer the excitement of chasing and being chased. Hide and seek is the most popular game of all. Dolls come next in order—imitation of family life next after desire for pure activity. After these two games the remaining active and imitative games are almost exactly equal in number. In all cases where a number of games were mentioned the one given first was taken as that preferred.

K. (1) Question : What is the earliest thing you are sure you can remember ?

The replies on the second questioning (97 cases) may be classified as follows :

Of 97 who answered:

- 17 remember a birth or death in the family;
- 14 remember being frightened or hurt, *e. g.*, falling down stairs, being stung by a bee, being bitten by a dog, being punished, getting fingers pinched, feeling pain in eyes from flashes of light through the windows of a railroad coach.
- 12 remember an illness of self or family.
- 10 remember an emotion; of these, three remember a feeling of grief or disappointment at the death of a pet animal, when parents started to the Centennial, when a necklace was stolen; one remembers a feeling of anger, and two remember the feeling of pleasure on receiving a present.
- 9 some novel experience which seems to contain an assertion of individuality or at least a recognition of self as acting independently, *e. g.*, going to Sunday school alone for the first time, walking across the floor the first time, the first sentence, running away, or acting upon a new idea strictly her own, such as dressing up a dog or trying to make a chicken swim.
- 7 something connected with grandparents.
- 2 remember a visit. These might be regarded either as belonging with the development of individuality or with the memory of new scenes.
- 1 remembers her father's singing.
- 25 remember particular scenes which do not fall into any of the arbitrary classes above. Of these:
 - 12 have central figures, *e. g.*, a Christmas tree, a fire, a horse and carriage, Barnum's giant, a colored man, a sword, a kitten, a chicken, a new bridge.
 - 5 have no central figures but consist of a number of sense impressions, *e. g.*, scenes at the Centennial, playing in the garden, the family moving, a wedding.
 - 5 while remembering scenes do not specify one above the rest.
 - 3 cannot select any memory as the earliest.

Taking the table as a whole several tendencies appear. The preponderant direction of the mind of the child is shown by the fact that seventy show attention to the outside world and only twenty-seven to self. Even when the child thinks of himself he is more apt to regard himself as a victim of sensations than as an agent in bringing things to pass; in the twenty-seven cases only eight are of self assertion. Two cases of self-discovery are interesting enough for specific mention. One respondent remembers sitting on her mother's lap and hearing her mother explain to someone that she must postpone a journey because her little daughter was ill. This gave her a queer feeling of self recognition. The other says I remember "standing in the middle of the floor in the back bed-room and thinking that I was one of the people like those

around me and not just looking on at the world." The ages in these two cases were respectively three years and between two and three years.

A superficial examination of the table seems to show that the child's world is a world of sensations rather than feeling (only five cases of emotion are specified) and chiefly of sensations of sight, but this is undoubtedly an error, for an emotion of some sort is evidently what made the experiences originally impressive. If in this later conscious recall sense elements predominate, it is because emotions are themselves not clearly attended to at the time they are experienced and not well recalled afterward. A child that is in terror of a dog is attending much more to the dog than to his own fears. The difficulty of recalling emotions has been fully recognized by psychologists. The preponderance of visual recollections may be due to a similar cause; sight is for most people the leading sense and, other things being equal, the focus of attention is turned upon what is seen. An interesting illustration of this general relation is found in the memories of illness where the things specified are the darkened room, the taste of the medicine, the candle lighted in the night, the mother's face bending over the sick child or the father's voice as he carried it, while the pain itself is not mentioned. The memory of pain, like that of emotion, is extremely colorless and imperfect.

(2) How old were you?

To this question eighty-nine replies were received, and the average was found to be 3.04+ years.

The different groups average as follows:

Memories of illness,	- - -	2.9+ years.
" " grandparents,	- - -	2.6+ "
" " birth or death,	- - -	3.3+ "
" " being hurt or frightened,	- - -	2.8+ "
" " special scenes,	- - -	3.1+ "
" " emotions,	- - -	3.1+ "
" " self-recognition,	- - -	3.2+ "

V.

Miscellaneous Questions.

As already indicated, the questions placed in this group proved less satisfactory than the others, and their results are chiefly valuable as warnings.

L. Mention some story that has made you weep—the most pathetic you can think of.

Replies on second questioning (90 cases): Thirty-nine (39) mention the death of some character in a novel; four

(4) misunderstandings ; three (3) hopeless self-sacrifice ; three (3) suffering for sin committed in ignorance ; two (2) disappointment of dearest hope in life ; seventeen (17) miscellaneous ; twenty (20) can select none ; two (2) never weep over books. The most interesting result of this table is the considerable number who specify cases not immediately connected with death.

M. Mention a funny story, incident, joke or scene in a book or play—the funniest you know, if possible

In response to this question many persons said they could not attempt to select the funniest story, because this was too dependent upon varying moods ; if they should select one, it would be merely the last funny story they heard. Many who did attempt to choose showed plainly that they mentioned the one freshest in memory, and no classification of them is possible.

N. What characters in history, fiction or life were ideals to you when growing up?

The answers to this question, so far as ideals are taken from history and fiction, is little more than an indication of what children read when most impressible. There is a decided and natural preference for ideals of the same sex, but a few respondents were careful to state that their ideals were of the other sex. Where the ideal is found in real life, the tendency is to idealise the less-known rather than the well-known, the teacher rather than the parents, and the old rather than the young.

O. If you had just one sermon to preach what would be your text?

Many subjects that were given in reply to this question could not be classified because so many sermons might be preached from them. Some hesitated to answer the question because they thought it might be asked to get an insight into individual character, and give the questioner a clue for later use. The first questioning was made during the Christmas holidays, when the financial stress was causing great suffering among the poor, and social questions were much discussed. This seems to have given color to the answers—at least in the second questioning answers of this kind were less numerous. Some of those who could not choose excused themselves by saying that their subjects would vary with their own meditations and could not be relied on as characteristic, and this is doubtless true of many who chose.

It is not difficult to find reasons for the failure of these questions. In *L* it was clearly a mistake to ask for the *most* pathetic story—thus introducing an unnecessary effort of comparison—and to specify the shedding of tears. If the request

had been given the simple form of F it might have been a tolerably good question. The same is true of M . Both N and O are ill-conceived in the matters for which they ask. If full answers had been obtained, only uncertain inferences could be drawn from them, and O is worst of all, in asking for something that a part at least of the respondents were disinclined to give.

So much for the results of this set of questions. On the questionnaire as a psychological method a point or two may yet be added. In the first place what tests are there, if any, for the exactness of the answers received? M. Binet, in his *Psychologie expérimentale*¹ after referring to the accord of the observations as a general control, mentions as tests applicable in special cases, simple experiments like requiring a respondent who has reported strength of memory for timbre to give the orchestration of a portion of a well-known piece of orchestral music, or one who has reported colored hearing to give at sufficiently separate times his photisms for a list of words. If the orchestration is well given, or if the double records of the list agree, the statements are probably true. The test of double questioning can be applied to any set of questions, in which it is possible, by lapse of time, or in any other way, to make the second questioning independent of the first. The amount of concordance in the two replies is a direct measure of the trustworthiness of the answer as a representation of what the respondent thinks on the matter in question; it does not of course show whether he is mistaken or not. A certain light is thrown on this last point by agreement or disagreement among the respondents, and (provided that answers may be fairly expected from all, as in the case of the questions above), by the number of those who find themselves unable to answer the question. If as many as fifteen in a hundred cannot tell how they force themselves to work when disinclined [$C(2)$], some uncertainty may be assumed in the answers of a good many who did answer. And if, as in G , two-thirds fail to answer, from unwillingness to reveal the causes of their anger, or for any other reason, it is highly probable that some at least of the other third were influenced in the extent of their confessions. When a questionnaire is sent out broadcast to the general public, this criterion cannot be used, though the total number of answers received to the simplest question of the set might serve in place of the total number of papers sent out.

The double questioning carried out in the case of the

¹P. 141 f., Paris, 1894.

questionnaire above, shows some wavering in the answers, even though the interval of two months was probably too short for complete forgetfulness; in most cases, however, the principal ratios of answers were not materially changed, and it is believed that they are substantially true. The inability of some respondents to tell how they recall a forgotten name, or how they set themselves to work when disinclined, shows that these questions approach the limit of casual introspection. The questions in Group IV met with the most ready and satisfactory answers. The respondents seemed always sure that they were making truthful reports, the recollections were interesting to them, and they were glad to make any contributions to a better understanding of the child.

In general, the questionnaire seems to the writer more valuable for the suggestions it gives the questioner than for its strictly scientific results. Each group of this study has suggested some problem for further investigation by experimental methods.

IX. THE MEMORY AFTER-IMAGE AND ATTENTION.

By ARTHUR H. DANIELS, PH. D.,

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The memory after-image is familiar to all who busy themselves with psychology; and many others have casually noticed it in their ability to count clock strokes from the beginning, after a number have already passed, or to pick up the whole of a sentence whose beginning has been neglected. The experiments of this paper were undertaken to determine, if possible, the duration of images of this kind.

A prime difficulty is to separate the simple persistence of the image (due, perhaps, to the native retentiveness of the nervous substance) from its continuation in associative memory. It is possible to avoid the latter by experimenting with completely distracted attention; for what is received with complete inattention forms few associative bonds, or none at all, and runs its course of gradual extinction without interference. The state of the image during the fading out can be discovered by requiring the subject at a given signal to turn his full attention upon it and to endeavor to reproduce it. The degree of his success will indicate the condition of the image. It is probably impossible, under ordinary circumstances, to secure perfect distraction, and, even if it were secured, it might not pre-

vent the formation of all associative bonds. Absolutely perfect distraction was not secured in the experiments about to be described; they were made, however, under as near an approximation to that condition as the experimenters were able to secure.

As a means of distraction, the loud reading of letters pasted on a revolving drum and seen, one at a time, through a hole in a screen, was first tried, but proved itself less effective than the reading of interesting stories in a loud voice and with the greatest possible rapidity. The image used was that aroused by the pronunciation of a group of three digits. When the subject was well underway in his reading, the digits were announced by the operator, the three occupying about two seconds. After a determined interval, at a signal from the operator (a tap on the table) the subject ceased reading and endeavored to recall the digits that had been given. The intervals were 0, 5, 10, 15 and 20 seconds. One hundred trials each were taken at these intervals, on each of two subjects, *S* and *D*, each serving alternately as subject and operator. At beginning, both subjects were somewhat experienced in this sort of experimenting and the trials, on different intervals, were so mixed as to exclude as far as possible the effects of both practice and fatigue. In spite of strenuous effort to concentrate on the reading it was not always possible to prevent a return of the digits to consciousness before the operator's signal and this difficulty increased with the increase of the interval. A record of the cases in

TABLE I.
Involuntary Return of Digits.

SUBJECT S.				
	5 Secs.	10 Secs.	15 Secs.	20 Secs.
No return,	47	29	22	19
One return,	50	67	66	54
More than one return,	3	4	12	27
SUBJECT D.				
	40	36	15	15
No return,	40	36	15	15
One return,	56	56	60	75
More than one return,	4	8	25	10

which such a return occurred was kept and is summarized in the table above. Since 100 trials were taken for each subject,

with each interval, the figures represent percentages also. At the zero interval the reply of the subject followed immediately on the announcement of the last digit, and no time was given for the involuntary return here considered.

The strong tendency which this table shows for the image to thrust itself into consciousness, was probably due in part to the knowledge that its recall would presently be required ; it testifies, in other words, to a partial division of attention. It is probably due in part, also, to the general tendency already often observed in both mental images and attention to a periodic rise and fall. When the original image of the digits was clear, the tendency to return developed almost at

TABLE II.
Decrease of Correct Replies with Increase of Interval.

SUBJECT S.

	0 SECS. ¹			5 SECS.			10 SECS.			15 SECS.			20 SECS.		
	Total Number of Cases.	Number Correct.	Per Cent. Correct.	Number of Cases.	Number Correct.	Per Cent. Correct.	Number of Cases.	Number Correct.	Per Cent. Correct.	Number of Cases.	Number Correct.	Per Cent. Correct.	Number of Cases.	Number Correct.	Per Cent. Correct.
No return,	100	56	56	47	5	9	29	0	0	22	0	0	19	0	0
One return,				50	7	14	67	1	1	66	1	1	54	1	

SUBJECT D.

No return,	100	96	96	40	24	60	36	10	28	15	0	0	15	0	0
One return,				56	40	71	56	17	30	60	7	12	75	7	9

¹With this record at the zero interval may be compared the following percentages of correct replies in the preliminary experiments, gotten by observers *D* and *A*, with no distraction (ordinary memory span) and with the distraction of reading letters from the revolving drum, in which case there was no interval between the pronunciation of the digits and the subject's response. The figures in parenthesis give the number of trials on which the percentages are based.

Subject.	NO DISTRACTION.			READING FROM DRUM.		
	8 Digits.	9 Digits.	10 Digits.	4 Digits.	5 Digits.	6 Digits.
A	100 (4)	78 (23)	31 (26)		53 (31)	7 (16)
D	80 (20)	63 (42)	8 (9)	74 (49)	27 (28)	26 (12)

once; if this was successfully resisted, the chances of complete exclusion were much increased. In what terms the return occurs—auditory or motor, or both—would be interesting to know, but cannot be stated with certainty; subject *D* is inclined to believe it motor in his case. Frequently, when there was no actual return of the digits, the subject was unmistakably conscious of their presence and their struggle for recognition, and by redoubled efforts at reading was able to prevent their re-entrance. This tendency to immediate and repeated return is undoubtedly of the greatest importance in ordinary memory. It is in this way that the original memory after-images are caught into the conscious memory trains and made recallable.¹ If no such return takes place a few seconds suffice to obliterate the image. A single return prolongs its existence, but for a few seconds only, as is clearly shown by Table II.

The table shows that under the conditions of these experiments the memory after-image does not last fifteen seconds in a reproducible condition, unless it is freshened by a re-entrance into consciousness. If it returns a single time it may outlast twenty seconds, but not often.

The table also shows a considerably greater persistence of the image in the case of *D* than in that of *S*, and the ordinary memory-span tests with *D* (see note to Table II) indicate a high retentiveness, but the chief cause was probably a less complete abandonment to the reading on his part. This was the impression formed during the experiments and is corroborated by the large number of *S*'s incorrect replies with the zero interval, where lack of retentiveness would play a small part. This can only mean that a certain minimum of attention is necessary for the original registry of the image, failing which its persistence is considerably abbreviated. During the experiments the subjects noticed differences in the vividness of the original registry, and found an extra effort at concentration necessary during the pronunciation of the digits, an effort which the operator also could often observe in the increased loudness of the voice of the reader. The announcement of the digits diverted a certain portion of attention to them, which, when the experiment was successful, was small and instantly withdrawn. The effect of this partial attention

¹The much greater persistence of what has been consciously in mind was also unexpectedly testified to by the errors made. Numbers accidentally met in the reading were apt, unintentionally, to take the place of forgotten digits in the test groups. This was also the case with numbers once used by the subject in replying. In one series of twenty groups, for example, the digit four was actually given five times, but occurred in the replies seventeen times.

is perhaps in the nature of a repetition, at least both subjects found it difficult to distinguish between repetition and good registry. The effect was most marked when the announcement of the digits occurred during a necessary pause in the reading. From all this it is therefore clear that the durations shown in Table II are in excess of what might be expected with perfect distraction, were that attainable. They represent a limit below which the duration of the simple memory after-image falls. With perfect distraction the subject ought to be unable, after a very brief interval, to say whether or not any digits at all had been announced.

A group of digits has, of course, a shorter duration than the single digits that compose it and is harder to reproduce. This is evident on a comparison of Tables II and III, in the latter of which is given the percentage of right answers for the digits in the first, second and third places. All replies (except those which had been preceded by more than one involuntary return of the digits) were used in making this table.

TABLE III.
Percentage of Right Answers for Different Parts of the Group.

	NO RETURN.			ONE RETURN.		
	1st Digit.	2d Digit.	3d Digit.	1st Digit.	2d Digit.	3d Digit.
5 Seconds,	14	29	87	20	24	80
10 Seconds,	24	13	27	5	14	73
15 Seconds,	9	13	31	7	10	60
20 Seconds,	5	10	15	14	9	40

SUBJECT D.						
5 Seconds,	70	70	95	78	78	95
10 Seconds,	30	41	83	62	39	76
15 Seconds,	20	6	46	31	21	75
20 Seconds,	10	10	5	29	17	66

The images of the separate digits evidently follow the same general course as the groups, but more slowly. Unless they get a recall, their chances of surviving fifteen seconds are, however, not very great. As regards the relative persistence of different parts of the groups, the table shows that the last

digit is more persistent and that there is little difference between the other two—a slightly greater persistence for the second in case of *S*, and for the first in case of *D*.

In conclusion, a few words may be added to show the relation of these experiments to those previously made by others. The term "memory after-image" (*Erinnerungsbild*) was introduced by Fechner,¹ who records observations on visual memory after-images. Exner² has described the same, under the name of the "primary memory-image," and gives examples for both sight and hearing. He remarks that the image vanishes in the course of a few seconds, if not caught by attention. Many experiments have been made on the simple memory span, with digits, letters or nonsense syllables. They have shown that, under favorable circumstances, groups of eight or ten members could be correctly reproduced. If the members of the groups were separated by one second intervals, this would show that in eight or ten seconds' time some of the members fall out of memory, and in even less time than that, for the errors are more frequent with the middle members of the group than with the first. The most elaborate experiments by this method were those made in Wundt's laboratory by Dietze.³ In these experiments groups of metronome ticks were compared with each other, on the supposition that accurate comparison is possible only when each group can enter consciousness as a whole. The size of the largest groups that can be accurately compared would then give the size of the greatest groups that can be taken into consciousness entire, *i. e.*, the groups in which the first member given is just fading out of memory when the last member enters. The supposition on which these experiments were made has been criticized, but apart from that they do not give sure determinations of the duration of the memory after-image, because of the very great effect of the rate at which the ticks are given and the tendency to rhythmical grouping of them. The experiments on memory-span have generally been made with full attention; Münsterberg,⁴ however, worked with attention distracted by solving arithmetical problems aloud, but used his results for other purposes than the measurement of the duration of the memory after-image. The experiments of Wolfe⁵ on the memory of

¹Elemente der Psychophysik, II, 491 ff.

²Hermann's Handbuch der Physiologie, II, ii, 282, quoted by James, Psychology, I, 646.

³Physiologische Psychologie, 4te Aufl., II, 288 ff.; for Dietze's full statement see *Phil. Stud.*, II, 362 ff.

⁴*Zeitschrift für Psychologie*, I, 1890, 99-107.

⁵Summarized by Wundt, *Phys. Psy.*, 4te Aufl., II, 431 ff.; for Wolfe's full statement see *Phil. Stud.*, III, 534 ff.

musical tones were aimed directly at the determination of this duration. In these experiments the ability to *recognize* a given tone was used as the test of the integrity of the memory-image instead of the ability to *reproduce* it which was used in the experiments of this paper. It is not surprising, therefore, that the duration found by Wolfe should be larger, extending to as much as sixty seconds. The fact that Wolfe's experiments were made with concentrated attention, and those of this paper with distracted attention, is also important, though single tones would not form many associative bonds, except perhaps with very musical subjects. The percentages of right answers are not directly comparable in the two studies, because of the greater opportunity for error with the groups of digits, but there is nothing overtly contradictory in them. The tendency of the digits to re-enter consciousness, observed by *S* and *D*, is undoubtedly the same that gives the periodic improvement of memory in Wolfe's curves.

X. ON THE LEAST OBSERVABLE INTERVAL BETWEEN STIMULI ADDRESSED TO DISPARATE SENSES AND TO DIFFERENT ORGANS OF THE SAME SENSE.

BY ALICE J. HAMLIN.¹

The figures commonly given by the text-books for this interval are taken from work done by Exner nearly twenty years ago and, so far as the writer is aware, since repeated only in part.² The object of the following experiments

¹Student at the Summer School, 1894.

²Exner, *Pflüger's Archiv.*, XI, 1875, 403-432. The statements in the text-books leave it open to the reader to infer that the figures are for single pairs of stimuli, (a single visual stimulus, for example, followed by a single auditory stimulus)—such at least was the conception under which these experiments were undertaken. When, however, Exner's original paper was examined, it was found that his method, except in the case of separate stimuli to the two ears, was such as to give him a series of pairs of stimuli instead of a single pair (pp. 403, 419-20, 423, 426). This may be represented diagrammatically as follows, letting (*a*) stand for auditory and (*v*) for visual:

(Auditory first).	<i>av.</i>	—	<i>av.</i>	—	<i>av.</i>	—	<i>av.</i>	—	<i>av.</i>	—	<i>av.</i>
(Visual first).	<i>va.</i>	—	<i>va.</i>	—	<i>va.</i>	—	<i>va.</i>	—	<i>va.</i>	—	<i>va.</i>

In the writer's experiments, on the contrary, single pairs (or triplets) of stimuli were used without exception, *e. g.*, either *av* or *va*. The importance of this difference will appear in the discussion of results below. The work of von Tschisch (*Phil. Stud.*, II, 603), of Angell and Pierce (this JOURNAL, IV, 528), and of Jastrow and

was a remeasurement of this interval for single pairs of stimuli under varying conditions of attention. An attempt has been made at the same time to notice accurately the subjective conditions, in the hope of finding by means of this introspection what the psychological basis may be for such discriminations. In most of the experiments only two stimuli were used, giving the following combinations: Eye and ear, eye and hand (electrical stimulation), ear and hand, right ear and left ear, right hand and left hand. The combination of right eye and left eye, upon which Dvorrák has made experiments, was undertaken by a fellow student and was consequently omitted. In a few experiments, stimuli to eye, ear and hand were used at once in a way which will be more fully explained below.

Apparatus and Method. The eye stimulus was always the flash of a small Geissler tube, the ear stimulus was generally the click of a telephone in the secondary circuit of a sliding induction coil (sometimes, however, the snap of an induction spark), and the hand stimulus a moderate induction shock in the tips of the middle and fore fingers. An almost unavoidable difficulty in using stimuli produced by induction apparatus is the variation in intensity. Reason will be given below for believing that moderate variations are without marked effect in such experiments as these. Nevertheless the variations were made as small as possible, and when the subject believed that he was influenced by them, the trial in question was discarded. The apparatus by which these stimuli were managed is fully described in the next of these "Minor Studies." It consisted essentially of a pendulum contact-breaker by which three electrical circuits could be broken at known and exactly adjustable intervals of time. These circuits in the further description will be called *a*, *b* and *c*. The range of possible intervals was from 0 to 44 σ ; those used varied from 18 σ to 44 σ . The exactness with which the intervals were kept by the instrument as shown by

Moorehouse, (this JOURNAL, V, 239), is unlike that of the writer for the same reason as Exner's. In the *Revue Scientifique* (XXXIX, 585) Bloch gives the results of experiments on this matter, but is so meager in the description of his methods that it is impossible to decide whether his results are comparable with those of the writer or not. Gonnessiat (*Recherches sur l'équation personnelle*, Paris, 1892, pp. 138-140) has measured the interval by which a visual stimulus may precede an auditory, and yet both seem simultaneous. And finally, Dr. F. Tracy has in this laboratory measured the just observable interval between a sight and a sound by a method practically identical with that about to be described. His results have been kindly placed at the disposal of the writer, and for them her acknowledgments are due. The results have been inserted below in their proper connections.

chronographic tracings was amply sufficient, the mean variation in no case being as great as one part in 100. In the experiments with two stimuli, it was customary to place the apparatus for one (*e. g.*, the flash) in circuit *b*, and that for the other (*e. g.*, the click) by parallel wiring in both *a* and *c*. It was easy then, by a simple switch, to throw the latter apparatus from *a* to *c*. If it were in *a*, the order of stimuli was *a b* (*i. e.*, click leading, flash following); if in *c*, the order was *b c* (*i. e.*, flash leading, click following). The break-key of circuit *b* was fixed permanently in a middle position. In adjusting the apparatus the keys for *a* and *c* were set to make equal intervals on either side of *b*. Except in a few preliminary experiments, the subject and operator were in separate rooms.

When the operator was about to give the stimuli, he sent a ready-signal by a telegraphic key and sounder, and then broke the circuits for the stimuli by dropping the pendulum. As soon as the subject had received the stimuli, he returned his judgment of their order in the same way. After a series of twenty judgments there was usually an interval of rest, or the operator and subject changed places. One hundred judgments (five groups of twenty each) were taken in nearly every case before the conditions were varied. The Method of Right and Wrong Cases was used, the subject being given the stimuli an equal number of times in each order; with the click and flash, for example, click-flash fifty times was mixed irregularly with flash-click fifty times. In cases of doubt the subject was required to guess. Of those who served as subjects, *S* was practiced both in general psychological experimentation, and in this particular kind of work; *D* had had general practice, but *Si* and *H* had had neither.

I.

Experiments with Unforced Attention.

The first thing to be undertaken was a study of the matter under normal conditions of attention. The general results of experiments on this point can be most briefly reviewed in connection with the following tabulated record :

TABLE I.

Group I. *Stimuli addressed to disparate senses ; attention at a balance.¹*

STIMULI.	Interval.	Subject.	Date.	Number of Tests.	Percentage of correct replies.	
					Flash first.	Click first.
Flash and Click.	*18σ	H	A 6	100	70	40
	29	S	A 9	"	73	67
	"	"	"	"	74	72
	* " H	"	"	"	66	42
	44	"	A 10	"	75	52
	"	"	"	"	84	62
					Flash first.	Shock first.
Flash and Shock.	44σ	S	A 11	100	73	86
	"	H	A 11-12	"	62	62
					Click first.	Shock first.
Click and Shock.	*18σ	S	A 7-8	100	40	74
	* " H	"	A 1-2	120	48	78
	30	Si	J 22-25	70	72	81
	31	H	J 21	100	67	87
					Snap first.	Shock first.
Snap and Shock.	44σ	S	A 11	100	62	96
	"	H	"	"	62	62

¹The experiments of Dr. Tracy, above referred to, were all of this type. The conditions were practically the same as those above described, except that the room in which the subject sat was partially darkened. His subjects were somewhat experienced in

Group II. *Stimuli addressed to different organs of the same sense; attention at a balance.*

STIMULI.	Interval.	Subject.	Date.	Number of Tests.	Percentage of correct replies.	
					Right first.	Left first.
Two Clicks.	18 σ	S	A 1	100	[85]	[77]
	"	D	J 24-26	"	[90]	[81]
	"	H	J 24-25	143	[59]	[81]
	"	"	J 26	105	[58]	[88]
	29	"	J 23-24	86	[84]	[81]
Two Shocks.	18 σ	S	A 2	100	66	74
	"	Si	A 4	"	58	64
	"	H	A 2	"	62	64
	*29	S	A 8	40	90	75
	* " "	H	"	"	85	80

The asterisks (*) indicate experiments omitted in making Table II below, A = August, J = July.

The experiments in which two clicks were used, bracketed figures in Group II, require a word of explanation. After experiments in this form were over, it was discovered that whenever the circuit of the right telephone was broken, a current was induced in the left circuit sufficient to cause a faint click

laboratory work, but had had little or no special training for this experiment.

Interval.	Subject.	Number of Tests.	Percentage of correct replies.	
			Flash first.	Click first.
13.7 σ	S	200	58	55
	Da	100	52	38
	L	100	74	44
	B	100	54	50
	Do	100	66	56
	T	200	79	71
50 σ	S	100	98	96
	Da	100	86	70
	L	100	66	68
	B	100	68	74
	Do	100	80	58
	T	100	94	92

in the left telephone. It might be supposed that this would tend to obscure the order of the clicks, especially when the right was given first, but this is believed not to have been the case. It is a well established fact that continuous sounds heard strongly by one ear and faintly by the other, are heard only on the side of the ear receiving the louder sound, and there seems to be no reason for thinking this untrue of clicks also. The fact that in the course of the experiments the presence of this faint induced click was never recognized is in accord with this hypothesis. A grain of evidence on the other side might appear to exist in the greater proportion of correct answers in the case of *H*, when the 18σ interval was used, but this is made doubtful by the non-appearance of the constant error with the 29σ interval. It seems a good deal more likely that with the shorter interval we have simply a case of well-marked constant difference. It is perhaps of interest to mention that both *S* and *H* hear more acutely with the left than the right ear, and *H* a good deal more acutely with either ear than *S*.

In order to facilitate comparison of the general results of this table, the intervals required to give 75% of right answers have been calculated according to the table in Cattell and Fullerton's work, "On the Perception of Small Differences" (p. 16), and are given in Table II. Since the object here is not

TABLE II.

Intervals required to give 75% of right judgments, compared with results of Exner, Tracy and Bloch.

	F-C	C-F	F-S	S-F	C-S	S-C	C - C		S - S	
							Left first.	Right first.	Left first.	Right first.
<i>Si</i>	32σ	37σ	48σ	28σ	(98σ)	(17σ)	16σ	12σ	19σ	30σ
<i>D</i>	—	—	—	—	35	23	—	—	34	60
<i>H</i>	35	169	98	98	48 (98)	19 (98)	14 15	9 44	— 34	— 40
<i>Exner.</i>	160	63	71	53	—	—	64	64	—	—
<i>Tracy.</i>	44	67	F = flash; C = click; S = shock. Figures in parenthesis under C-S and S-C were gotten with a snap of an electric spark instead of the click of a telephone.							
<i>Bloch.</i>	28	36								

the exact record of results, but a picture of the phenomena under examination, some series of tests (those starred in Table I) have been omitted, four because the size of the interval used did not give results suited for calculation, and two because the number of tests was small; all other series of Table I have been included. With the same end in view, the results from tests made with different intervals have been combined. It hardly need be said that no importance is attached to the precise figures thus calculated, and no statements are made with regard to them that are not justified by the original records. The records of Exner, though made under different conditions and by a different psychophysical method, together with those of Tracy and Bloch are added for comparison.

The most noticeable thing in this table is the frequency of large constant differences; *S*, for example, in the combinations of flash and shock, requires a considerably larger interval if the order is F-S than if it is S-F, and *H* requires a much greater interval for C-F than for F-C. Some of these are evidently personal differences, one observer succeeding best with one order, the other finding no difference or succeeding best with the other. Reference to Table I shows also that some of the differences seem to depend on the general difficulty of making any judgment, and disappear, or even take the opposite sign when the interval is increased. This, at least, is the case for *H* in the C-C experiments and for *S* in the S-S experiments.

A tolerable unanimity, however, was found with the flash-and-click combination; the interval required for C-F was always larger than for F-C. In several series the difference is so slight that it may be accidental, but in others it is marked and in all there is no contrary case. In Tracy's twelve series there are only two instances of differences in the contrary direction, and these are slight in amount. In the experiments of Bloch, if the two studies are comparable, the same is to be observed. This is the more interesting as it is flatly opposed to the direction of the constant difference as observed by Exner in all seven of his subjects.¹ The relation is too constant in both cases to be set down as mere accident, and must be referred to some variation in the conditions of experimentation. The variations are so numerous that it is hard to fix with certainty upon the particular one, but three

¹ Gonnessiat also concludes from his experiments that the visual stimulus (the instantaneous flash of an artificial star) must precede the auditory (telephone click) by about 50 μ , in order that they seem simultaneous, thus supporting Exner.

have a certain plausibility. In the first place, Exner's records are all naïve; in order to equalize the effect of practice, all practice was excluded and short series were taken on unpracticed subjects. In the long series of Tracy and the writer, the subjects could not remain unpracticed, and it may be that with increasing expertness changes in the direction of the constant differences occur. The records of the writer's experiments, while not very conclusive on this question, do not give evidence of such a change. In most of Exner's experiments, also, attention seems to have been given chiefly to one stimulus or the other, while in the writer's it was held in balance. It seems more likely, however, that the contrary direction of the constant difference depends on the fact that series of pairs of stimuli were used in one case and single pairs in the other.¹ In his experiments on personal equation, Gonnessiat finds rhythm a very important factor, and it may have been effective in these experiments of Exner's. Why Gonnessiat himself should have gotten such results as he did in trying to make the two stimuli seem simultaneous, is not easy to explain from data at hand, but it is highly probable that getting a coincidence of the two sensations is quite a different process from determining which comes first.

Whatever the cause may be, the fact remains that for single pairs of stimuli and for many observers the order F-C requires a less, or at most no greater interval than the order C-F, and this necessitates a revision of the explanation offered for the constant difference by Exner,² which makes the difference depend on the slower rise and greater duration of the visual sensation. The constant differences also that occur in the case of the click and shock, and especially those in case of stimuli to different organs of the same sense, cannot well be made to fit with an explanation based wholly on sensory inertia. It is more natural to refer the phenomenon, as Wundt does chiefly (*Phys. Psy.*, 4te Aufl., II, 392), to differences in the direction of attention, either habitual or depending on the nature and intensity of the stimulus, the sensation receiving preponderant attention appearing to come first. Habitual attention to what may be seen, in preference to what may be heard, is characteristic of many minds. The electric shock in the touch experiments, also, though not distinctly greater in intensity than the flash and click, would probably be regarded by most subjects as more intrusive.³

¹*Op. cit.*, pp. 430-431.

²*Op. cit.*, p. 425.

³A delay in the response of the telephones, if such there were, might explain some of the cases, but such a delay is practically out

II.

Experiments with Forced Attention.

Experiments on Voluntarily Directed Attention. The experiments were undertaken in full expectation of finding a definite relation between the direction of attention and the apparent order of the stimuli. Several short series of experiments were made with different pairs of stimuli, (a total of 360 for *S*, and of 278 for *H*), but nothing like a definite connection could be made out, and the experiments were broken off. So far as the figures showed anything, they showed that forced attention was as often a hindrance as a help, even when the leading stimulus was attended to, and that attention to the following stimulus was often advantageous. A good deal of support is given to the conclusion that voluntary attention is ineffective by the general observation that the best results were reached when the subject was in a state of indifference. Of this, more will be said presently.

Experiments on Involuntarily Directed Attention. Experiments with stimuli of different intensities were made in the expectation that the fainter stimulus could be so far reduced as to compel the subject's attention involuntarily to it through his fear of losing it. This seems to have succeeded with *H*; for the faint sound was often lost altogether, and she felt a straining of the attention towards the weaker stimulus. Yet, when heard, it usually seemed to lead and gave the subject 80% right when the weak sound led, to 32% when the loud sound led. The record of *S* showed the opposite tendency. He seems not to have felt the compulsion toward the weaker stimulus, but allowed his attention to balance indifferently between the two until one sound was made very faint, and then felt only an occasional tendency to listen for the weaker sound. When the stronger stimulus led, this subject had 92% of his judgments right, when the weaker led, only 42%. Differences like these seem to indicate an individual caprice of the attention. It is caught in one

of the question. With the apparatus at hand, it was not possible to measure the delay of the telephone plate when it moved as little as in these experiments. Chronographic tests, when a stronger current was used showed no appreciable delay. No regard was had in the experiments to whether the first movement of the telephone plate was toward the ear or away from it, but the differences in time introduced by this must be extremely small. This problematic influence of the apparatus was completely avoided, when the snap of an induction spark was substituted for the telephone click. With *H* this seemed to make a difference; with *S*, however, the constant error remained as before.

case by a loud sound, in the other by a faint sound. And the individual tendency is so persistent, that even when a subject knows that his attention has been partial to one stimulus, he cannot correct the tendency. All these attention experiments seem to show, in a single word, the helpfulness of spontaneous as opposed to voluntary attention.

Besides the experiments already mentioned, a few records were taken when three stimuli were given the subject. The click and the shock were sent at the same instant, and separated from the flash by an interval of 18σ . The subject recorded the apparent order of the three. Both subjects found that the click seemed to lag behind the other two stimuli, and noticed a growing tendency to ignore the sound altogether, making the experiment practically a repetition of the flash and shock tests, at a shorter interval.

A certain interest may attach to the following general observations:

When an observer is practiced the interval between the stimuli sometimes seems quite an extensive period of time. In judging the order of a flash and click at an interval of 18σ , *S* noticed several times that he distinctly waited for the second stimulus. When the flash came first he had a definite sensation of darkness before hearing the click. *H* noticed something similar at times. When listening to the two clicks at an interval of 30σ , this subject had a muscular sensation apparently intervening between the two sounds, as if the interval had been long enough for the attention to shift its muscular adjustment after receiving the first, before taking in the second.

The interpretation of the successive stimuli in terms of motion, noticed by Exner was frequently observed in these experiments. It was prominent in the two shock experiments and in those with the flash and shock, but with the two clicks it was much fainter or entirely absent.

III.

Conclusions.

During the progress of the experiments the chief subjects were on the watch for introspective suggestions, but introspection under the conditions of this experiment proved unusually hard, and not much was obtained in this way. A little light is furnished, however, by an observation, partly introspective, partly external, which was early made by Tracy and was repeatedly confirmed in the writer's experiments, namely, that the best results were reached when the subject assumed a certain indifference, awaited the stimuli

without strained attention and based his reply upon a "general feeling" of their order rather than on a clear recognition of it. The condition of mind may be most accurately designated as "alert indifference," a condition in which voluntary effort furnishes the general ground, but nothing more. The subject was always in an easy and unconstrained position, but the muscles of the head, of breathing, of the eye, ear, or hand, were somewhat innervated. This was occasionally shown by some slight muscular reaction; the hand would twitch in response to a very faint electric shock, or the eyes would wink when the flash came. But along with this degree of sensitiveness or alertness, there was a mental nonchalance. The subject was in a passive state, free from any sense of care or effort, and with no lasting memory of what was taking place. If a judgment was not recorded at once, it could not be given at all.¹

The hypothesis which the experiments have suggested with reference to the psychophysical mechanism by which such judgments of order are made is briefly as follows: The sudden entrance of any stimulus causes an immediate reflex response of adjustment in the organ in case of the eye and ear, and perhaps of withdrawal of the member in the case of the hand. What is really compared in judging the order of the stimuli, is not the special sense impressions, but the sensations resulting from these reflex movements. In order that these reflex responses may be prompt, the subject must be alert, but must not be voluntarily attentive, because voluntarily attention causes beforehand a more or less complete adaptation of the organs in question, and thus obscures the reflex response. The problem would then become one of the necessary length of the just observable interval between two sensations of movement, a problem on which, so far as the writer knows, no other data are extant.

This conception of the mechanism and the observations on which it rests, agree well with the first of Exner's several types of attention. The first is thus described: (p. 429) "We adjust our attention for the first stimulus that is to reach us, of course without knowing which it is; and not for this alone, but also — I cannot express myself differently — for the condition of the sensorium at the instant of this first stimulus. By this adjustment that instant is fixed in memory, and which of the two stimuli was the one adjusted

¹To *S* it sometimes seemed that there was a certain tendency to regard the sensation which seemed stronger in consciousness as the first, if the judgment was made from recollection and not from immediate sensation, but he is unwilling to attach much importance to this observation.

for can be recognized in the memory image; that one is then the first. The second stimulus is wanting in this memory image in so far as the image results from an exact adjustment. The limit of the discriminable is reached, when it is no longer possible to fixate the first stimulus alone." This method was noticed by Exner only in the case of the separate stimuli to the two ears, and, while he thinks it may possibly have occurred in the other cases also, he is inclined to believe that the close resemblance of the two stimuli is an essential condition of its development. This restriction seems doubtful; at least, in the experiments of this study, no difference in type was noticed in the different combinations of stimuli. It is much more probable that this type is characteristic of experiments in which a single pair of stimuli are judged, and that the type which Exner found for stimuli to disparate senses belongs to the rhythmically recurring pairs of stimuli. As already explained, the two click experiments of Exner were the only ones where his apparatus seems to have allowed the production of a single pair.

The results of this study may be summarized briefly as follows:

1. The interval that must separate instantaneous stimuli addressed to disparate senses, or to different organs of the same sense in order that their order may be recognized, has been measured for single pairs of stimuli, and by a method as nearly as possible the same in all cases. The results of this measurement, besides indicating some changes in the figures commonly given for these intervals, make the explanation of the constant errors, found with the click and flash, by optical inertia apparently unnecessary.

2. The effect of voluntary attention has been examined, and so far as the experiments go, has been shown not to cause the stimulus for which attention is set to seem to lead in time. Throughout the experiments, on the contrary, the importance of spontaneous attention, or at least the spontaneous reaction of the psychophysical mechanism has everywhere appeared.

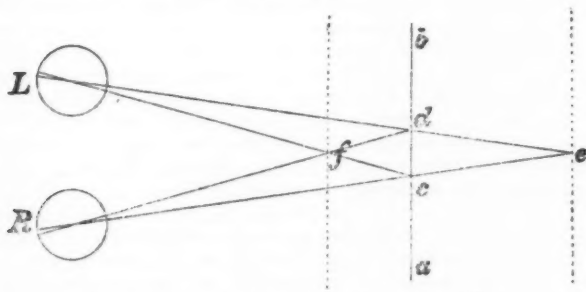
XI. NOTES ON NEW APPARATUS.

BY EDMUND C. SANFORD.

The Binocular Stroboscope. The purpose of this note is the double one of calling attention to a little known phenomenon of binocular vision and describing an instrument by which it

may be demonstrated. The phenomenon was long ago studied by Dvorák, but his paper is hidden away in the Proceedings of the Bohemian Royal Society of Sciences,¹ and has received little notice. The only contribution of the present writer is such a simplification of the instrument as makes the phenomenon demonstrable in any laboratory that has a vertical color-wheel and a mirror. The phenomenon in question is, as the title of Dvorák's paper indicates, a sort of "personal equation" between the two eyes. If the right eye receives a stimulus and a little later the left eye receives another, the two stimuli, if close together, may seem simultaneous and be credited to a single cause, or, if further apart, may be recognized as separate and credited to separate causes. Quantitative measurements of the time that must separate two such stimuli were made by Dvorák and some preliminary work in the same line has been done in this laboratory.

The point of present interest is, however, not this general case, but the particular one in which the stimuli are separate glimpses of a moving object. When proper conditions are observed such successive glimpses are united into an illusory perception of distance. The nature of this illusion will be made clear by the following diagram:

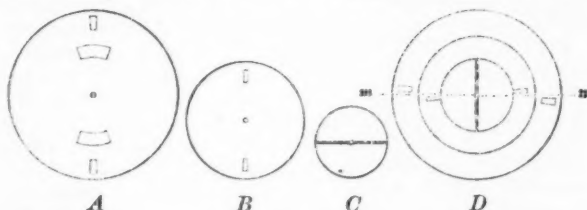


R and *L* represent the right and left eyes; the object moves in the line *a b*. Suppose in the first place that the object starts from *a* and moves upward, and that when it reaches *c* it is seen for an instant by the right eye and when it reaches *d*, for an instant by the left. The united perception will then be located at the intersection of the lines of sight, that is at *e*. If the order in which it is seen by the eyes is reversed (the

¹ Ueber Analoga der persönlichen Differenz zwischen beiden Augen und den Netzhautstellen desselben Auges. Sitz.-ber. d. k. böhm. Gesells. d. Wis. in Prag; Jahrgang, 1872. Jan-Juni, pp. 65-74.

direction of motion remaining the same), the left eye will see it at *c* and the right eye at *d*. In this case as before it will seem to be at the intersection of the lines of sight—this time at *f*. This is true for both direct and indirect vision, and gives a certain support — probably more apparent than real—to the old projection theory of visual localization.

The simplest means of getting such instantaneous glimpses of a moving object is to use a disk with radial slits as in the ordinary stroboscope. The instrument is easily made binocular by cutting separate slits for the two eyes, and both binocular and adjustable as to the order in which the eyes are used, by applying a smaller disk concentrically over the first. *A* and *B* in the diagram below are a pair of such disks.



The moving object in Dvorák's apparatus was a broad band of paper with vertical stripes, placed a little distance behind the disks and driven by the same means as they. In the simplified instrument the moving object is a figure of some kind painted either directly on the smaller disk or on a third still smaller disk placed upon the same spindle as the other two. Such a figured disk is represented by *C*, and the moving object is the black bar which it carries. In *D* the disks are shown combined for use.

In using the instrument the combined disks are placed on the color-wheel, and at a convenient distance before them a mirror so set that the observer, looking through the slits from behind into the mirror sees the reflection of the face of the disks. The bar of the smallest disk should be vertical when the diameter that halves the angle between the slots, *mn* in *D*, is horizontal. The instrument may be used without further addition, the eyes being brought close to the slits at about the height of the spindle. It is better, however, to place between the eyes of the observer and the slits a screen of black cardboard with a narrow horizontal slit (placed radially with reference to the disk) so as to prevent the observer from seeing through the slits except when they are immediately before his eyes. When the instrument is thus set up, and the disks are so

adjusted that the right eye sees first, and the direction of rotation as seen in the mirror is like that of the hands of a watch, the observer sees the image of the bar inclined toward him at the upper end and away from him at the bottom. When the left eye leads the inclination is reversed. The effect is most striking when the lead of one eye over the other is rather small, for otherwise the positions of the bar are too discordant for easy spatial interpretation; a lead of five or six degrees is sufficient.¹

The size of the disks must of course be varied to suit the machine upon which they are to be used. The following dimensions will probably be found convenient on most color-wheels: Radius of large disk, 15 cm.; of small disk, 10.5 cm.; of figured disk, 6.5 cm. Distance of outer edge of narrow slits in large disk from centre of disk, 14 cm.; of inner edge, 11.5 cm. Distance of outer edge of broad openings from centre of disk, 9 cm.; of inner edge, 6.5 cm. Distance of outer edge of slits in smaller disk from centre of disk, 9 cm.; of inner edge, 6.5 cm. Extent of broad openings in large disk, 40° ; of narrow slits in both disks, 5° . If the slits are made too narrow the image of the bar is clear cut, but weak in illumination; if too broad the image is stronger but blurred. In other forms of the instrument it is often convenient to use more than two slits in each disk, but in this it is a disadvantage, for with more slits the bar is seen more frequently and in positions where the separate glimpses are not capable of a spatial interpretation. The result is a case of irreducible double images, as may be seen by using the disks when the bar of the figured disk is brought into the diameter that halves the angle between the slits.

Other distortions of the image of the disks in the mirror can sometimes be observed, but they can for the most part be easily explained on the principles already set forth.

A Model of the Field of Regard. The movements of the eyes and their effect on visual localization are an interesting, but somewhat difficult topic in the psychology of vision. One

¹Another satisfactory figure is one which consists of a couple of heavy black rings placed near where the ends of the bar now lie in C. When this figure is used and the right eye leads, the upper ring will look smaller and nearer, the lower one larger and further away. A still more interesting case, but one which requires an independent moving object, is that in which the object moves in a horizontal circle, a vertical wire, for example, moving upon a circle three or four inches in diameter. When seen through the disks its path seems to be elliptical, the apparent direction of the long axis of the ellipse and the direction of the motion depending, in part, at least, on the order in which the eyes are used.

difficulty is getting a notion of what the hemispherical field of regard looks like and what its relations are to the plane field on which experiments are generally made. To assist in removing such difficulties as these, the model about to be described was constructed. A stereoscopic picture of the part that represents the hemispherical field, and an ordinary diagram of the part that represents the corresponding plane field will be found in the appendix to the section of laboratory experiments on the Visual Perception of Space given below. A detailed account of what the model presents is given at the same place and may be omitted here.

The general plan of the model will easily be understood by reference to the stereoscopic figure. The framework is of wood, its most important portion being the face board—that carrying the letters—which is twenty-eight inches square—seven eighths of an inch thick, and has a twenty-four inch circle cut from its centre. To this are fastened at *A*, *B*, *C* and *D* semi-circles of brass of two-inch radius and three sixty-fourths of an inch thick. To these were first soldered the prime meridian *AB* and the equator *CD*. The wires used were of iron and about three thirty-seconds of an inch in diameter. The wire when bought was coiled with a radius nearly equal to that of the hemisphere to be constructed so that little bending was necessary. The remaining meridians were next soldered in their places upon *CD* and on the brass plates at *A* and *B*, and after them the parallels and the oblique circles *EF* and *GH*. The crosses, cut from tin, were then put in place and finally the small circles *IJ*, *KL*, *MN* and *OP*—Helmholtz's Circles of Direction—and the whole completed by painting the face board and wires a dull black.¹

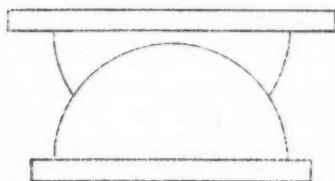
It was not at first intended to have the model show the relations between the hemispherical and plane fields of regard. In the present form of the instrument this has given place to a wide and thin board which slips into place behind the wire hemisphere and stands for a plane tangent to the latter at the middle point of the central cross. On this has been drawn a gnomonic projection of the wires and crosses of the hemispherical field, in other words, the figures that would be made by the shadows of these parts cast by a point of light in the centre of the lettered circle. These projections may be gotten mathematically by calculation or empirically by actually casting the shadows and tracing them. The diagram in the appendix was drawn by calculation, but

¹The white letters were made by tracing from a stencil on bits of cardboard and then painting over all other parts with black.

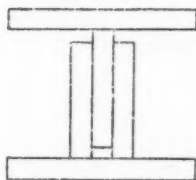
with the actual model the shadow plan was chiefly used as promising to accommodate itself more easily to small accidental irregularities in construction. By this combination of the plane and hemispherical fields the student can instantly satisfy himself that what he sees in one field is in reality an exact representation of what he sees in the other by bringing his eye to the centre of the hemisphere and seeing that the lines coincide.

The instrument as described is large and suitable for class demonstration. An instrument one quarter the size would answer equally well for individual inspection and would be much less cumbersome; indeed, the stereoscopic diagram, in connection with the corresponding diagram of the plane field, serves almost every purpose. The writer has had a few extra copies of both struck off, and will be glad to furnish them, as long as they hold out, to any one interested in them.

A Simple Adjustable Stand. This piece of apparatus is easily within the skill of any one that handles ordinary tools. Its plan will be clear from the diagram below. It consists of a base board twelve inches long, eight wide and seven-eighths of an inch thick. Lengthwise of this and seven-sixteenths of an inch on either side of its middle line, are placed two vertical semicircles of wood, of five-inch radius and seven-eighths of an inch thickness.



SIDE PLAN.



END PLAN.

The upper part of the instrument is like the base board, except that it has a single semi-circle of wood along its middle line, and that it is a little larger—fourteen by ten inches.

When the instrument is put together, the semicircle of the upper part slips in snugly between the semicircles of the lower part, and the whole is fastened in any required position by means of an ordinary iron clamp, which squeezes the lower semicircles against the upper. The instrument made

in this way allows an angular adjustment of 90° in either direction from its middle position, and a vertical adjustment of three inches or more. When the clamp has just brought the sides together, the parts slide upon one another, and will retain any position given them, but when the clamp is screwed solid, the whole is as rigid as if made of a single piece. It is, of course, not necessary to use semicircular boards for connecting the base board and the top; almost any shape will serve, and some other shapes give even a greater range of adjustment.

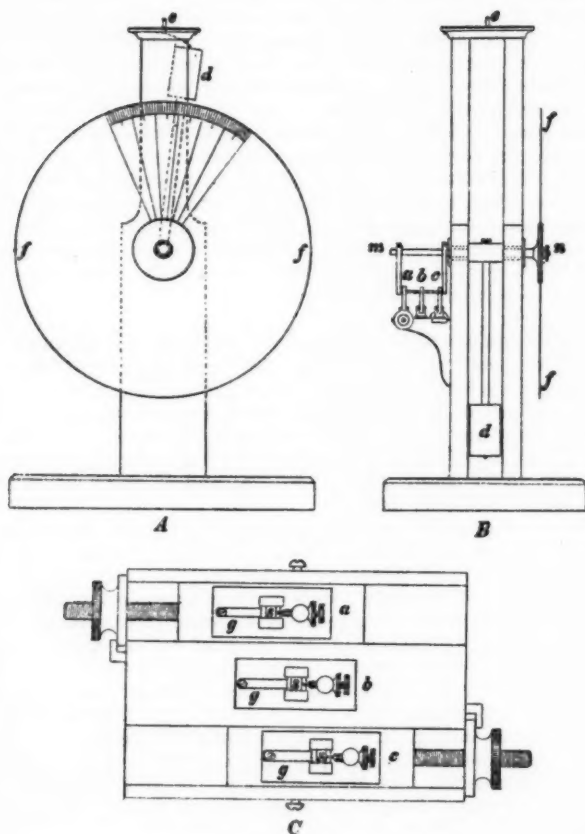
The stand was first designed to serve as an arm rest when it is desired to support the arm without keeping it absolutely motionless. For this purpose, a square bottomed, opened-ended trough of wood is used on top of the stand. This trough is twelve inches long, six wide and four deep, with a slack piece of cloth tacked across the top. The usefulness of a stand with such a range of adjustment, however, is obviously not limited to furnishing an arm rest.

The Pendulum Circuit Breaker. It was the purpose of this instrument to break three electrical circuits at known, and regulable intervals of time from one another. The familiar method of a swinging pendulum was used, but some adaptations have been made which have proved convenient in use and may justify a description. The instrument is that referred to in Minor Study, No. X, and is pictured in the cut on the following page:

The pendulum (*d* in Figs. *A* and *B*) is of brass and swings completely over on the axes *mn*, between the two upright posts of wood. The latter are of pine, one and one eighth of an inch thick, five inches broad at the bottom, and two and three quarters at the top, securely fastened into a base of the same material (eighteen inches long by twelve wide), and further braced together by a wooden cap at the top. The pendulum is a foot long and weighs, rod and bar together about three pounds. It is dropped from the nearly vertical position seen in Fig. *A*, by pulling back the release *e*. The keys are not struck by the pendulum itself as is common in such instruments, but by a striking bar or frame-work of brass, extending downward from the shaft *mn*, shown in contact with the keys *a*, *b* and *c* in Fig. *B*.

The other end of the shaft *mn* is finished like the spindle of a color-wheel for receiving a large disk *ff*. As used in Study No. X, this disk was of tin and about seventeen inches in diameter. Three functions may be performed by the disk: It furnishes a surface that can be smoked over and used for timing the pendulum chronographically; it furnishes a means,

when pierced with a radial slot and illuminated from behind, of producing a very brief flash of light; and, by the scale of degrees scratched on its edge, it furnishes a means of setting the keys without the use of their micrometer screws. In



Study No. X, it was used only for the first and last purposes, the flash being produced in another way.

Fig. C gives a plan of the keys as seen from above. Keys *a* and *c* were movable, key *b* was fixed in a central position,

so chosen that the striking bar was just in contact with it when the pendulum was at rest in its middle position. The shelf on which the keys stood was of iron, and each had to be insulated from it; they were, therefore, set on pieces of hard rubber, *g, g, g*. The upright arms of the keys are lettered *s, s, s*, and each was so shaped at the bottom that when it was erect, it was held in place by the spring (near the *g*'s in Fig. *C*), and when it was thrown down it was prevented by the same spring from rebounding. On either side of the shelf was a set screw for fastening the keys in place when once adjusted. The micrometer screws attached to *a* and *c* could have been used for setting them, but it seemed better in Study No. X, to use the screws for making small changes in the position of the keys, and to do the setting directly from the disk as already mentioned.

The setting was accomplished as follows: The disk *f* being clamped tightly in place, a fixed point was fastened to the base of the instrument and brought close up to the lower edge. The keys *a, b* and *c* were connected with circuits in which were telephones or other apparatus for announcing the instant at which the respective keys were struck and their circuits broken. The pendulum was then lowered by hand and carried slowly by its middle position, and an exact reading obtained of the point on the degree scale at which the click announced that a key was struck. The whole degrees were shown by the scale, the tenths were estimated by eye. By chronographic measurement it had previously been determined that one degree corresponded to 1.6σ , and knowing this it was easy to set the keys to any required interval.

In using the instrument for the production of two nearly simultaneous stimuli in reversible order as required in Study No. X, the keys were so connected with the stimulation apparatus that the break at *b* gave one stimulus, *e. g.*, the flash of a Geissler tube, and that a break in either *a* or *c* (by parallel wiring) gave the other, *e. g.*, a click in a telephone. Whether the click should lead or follow the flash, was then controllable at will by a switch in the hands of the operator, all three keys being set up each time.

The value of such a piece of apparatus depends on its accuracy. The following records of chronographic tests with a Deprez signal and tuning-fork, giving approximately 149 vibrations per second, show a degree of accuracy very satisfactory for all purposes for which the instrument is likely to be used. The table gives the average number of vibrations of the fork for the 60° at the bottom of the pendulum's arc:

Table Showing Average Rate of the Pendulum.

Date.	Number of Trials.	Average. No. of vib.	Mean Variation.	Maximum.	Minimum.
J-7	22	14.13	0.06	14.25	14.00
J-22	34	14.15	0.07	14.30	13.95
A-3	14	14.13	0.09	14.30	14.00
A-3	12	14.08	0.05	14.20	14.00

Whether the keys were raised so that they were struck by the pendulum, or were turned down so that it swung free, made no appreciable difference in its rate. Of the two sets of tests on August 3, the first was taken after the instrument had been used for several days without re-oiling, the second immediately after fresh oiling. The oiling appears to have reduced the time for 60° by about seven one hundredths of one tuning-fork vibration (roughly 0.5σ), or about one part in two hundred.

The advantages of this instrument are the swinging of the pendulum completely over and the attachment of the disk for chronographic control of its rate and accuracy. The pendulum rises well up at the end of its swing, and is easily caught by hand and carried on till it rests again at *e* ready for another fall. The complete swing makes the apparatus compact by bringing the catch from which the pendulum falls above the axis instead of at one side, and also avoids the backward swing which is often a considerable inconvenience in the common form of the pendulum chronograph. The advantage of the disk over a plate fastened to the pendulum itself, is the greater ease of adjusting writing points to a surface with which they are constantly in contact.

A pendulum of this kind with the attached disk is probably the simplest and cheapest time apparatus at present attainable for reaction-times or any other brief time intervals. With a disk permanently scratched with lines corresponding to hundredths of a second (following an idea suggested by Bowditch for a similar purpose) or even thousandths, determinations could be made both rapidly and accurately, especially if one of the keys *a*, *b* or *c*, were used for giving the stimulus so that the time to be measured should always begin at the same place in the swing.

ON THE WORDS FOR "ANGER" IN CERTAIN LANGUAGES.

A STUDY IN LINGUISTIC PSYCHOLOGY.

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In such works as Darwin's "On the Expression of the Emotions in Men and Animals," Romanes' "Mental Evolution in Man and Animals," Mantegazza's "Fisionomia e Mimica," "Fisiologia dell' Odio," etc., one looks in vain for a section devoted to the examination of the concepts of the various emotions as revealed by the terms used by the ruder and more primitive peoples to denote them. In connection with the theory of the emotions put forth by Prof. James, and as illustrations of the intimate kinship of psychology and philology, such investigations are of no little interest. Studies of the emotion of love from this stand-point have been made by Dr. D. G. Brinton (3), for certain American aboriginal tongues, and by Dr. Carl Abel for Latin, Hebrew, Russian and English (1).

So far as the present writer is aware, no attempt has been made to discuss the subject of anger, and this brief essay is intended a beginning in the sematology of that emotion. First, let us glance at our own language. *Anger*, which in Middle English meant "affliction, sorrow, wrath, pain, inflammation" — we still speak of "an *angry* wound or sore," and the familiar phrase of Shakespeare "more in sorrow than in *anger*," preserves traces of the kin of grief—is borrowed from Scandinavian, and, with its cognates: Icelandic *angr* (grief, sorrow); Danish *anger* (compunction, regret); Swedish *anger* (repentance, penitence, regret, compunction, sorrow—the adjective *anger* signifies "afraid, sorry"); Latin *angor* (strangling, throttling, quinsy, bodily torture, anguish, vexation, grief, sorrow; *angere*, to compress, stifle, choke, strain, strangle, throttle, twitch, gripe, trouble, torment, vex); Greek *ἀχος* (ache, pain, distress — used in Homer of the mind only; *ἀχῶ*, "I mourn, am sad at heart, grieve, I vex, distress, make to grieve"; *ἀχῶ*, "I press tight, press the throat, strangle, throttle,") etc., go back to a primitive Indo-European radical *agh* (*angh*) expressive of the very physical idea, "to choke, to oppress, to constrict." Our common English phrase "choked with anger" is really tautological at bottom, for *anger* once meant *choking*. Other interesting words which have sprung from the same root are: *awe* (fear, dread), cognate with Icelandic *agi* (terror), Danish *ave* (check, control, restraint); Gothic *agis* (fear, fright, terror); and (probably) Sanskrit *amhas* (pain), *agha* (sin); *ugly* (frightful, hateful — compare German *hässlich*; we speak of "an *ugly* temper," and in American English *ugly* signifies "ill-tempered, gross-grained, vicious"); cognate with Icelandic *uggtir* (fearful, frightful, dreadful, to be feared; *y'gtigr*, terrible; *y'gr*, fierce), *uggr* (fear), *ugga* (to fear), *ógn* (ter-

ror), *ġna* (to threaten); Gothic *ġan* (to fear), *oġan* (to terrify); all of which are from a base *ag* or *og*, "to fear," itself a derivative from *agh* in the larger sense indicated above. Our *ache* is also related, a memory of which is yet present in the assertion of the school-boy preparing to assail his opponent, "I'm just *aching* to get at you." Another shoot from the same stock is German *Angst* (anguish, anxiety, fear, pang), a word apparently occurring only in the High German dialects — Gothic has, however, *aggwītha*, "anguish" — and related to Latin *angustia* ("narrowness, straitness, difficulty" — whence French *angoisse*, our *anguish*), and German *enge* (narrow, restricted), *bange* ("anxious, afraid," from *be-enge*), which latter word in Middle High German was also a substantive, with the meaning "sorrow, anxiety."

Another English word for "anger, indignation" is *wrath*, a substantive, derived from the Anglo-Saxon adjective *wrād* (whence also our *wroth*, "angry, wrathful"), and cognate with Icelandic *reiði* (wrath). The correlation of *wroth* is with Icelandic *reiðr*, Danish and Swedish *vred* (angry, wrathful); Dutch *wreed* (cruel, hard, harsh), and *reit*, which in Middle and Old High German meant "twisted, curled." In truth, the Anglo-Saxon *wrād* is but the past of *wriðan* (our *writhe*, "to twist to and fro," Icelandic *riða*, Danish *vide*, Swedish *vida*, Old High German [the word is lost in the modern tongue] *ridan* "to wring, twist, turn, wrest, to wind"); going back to an Indo-European root *uert*, seen also in Latin *uertere*, "to turn, twist." A man *wroth*, then, is literally one whose mind or body is "turned, twisted, awry."

Our word *cross* (ill-tempered, angry), in Middle English, *crous*, finds its cognates in Dutch *kroes* (curled, crumpled, confused, cross, stubborn); Modern German *kraus* (curly, crinkled, crisp, etc.). The Low German proverb: "Krüse hār un kruse sin, dār sit de düfel drēmal in," brings out the same idea in curious fashion, and Martin Luther delighted to hurl at one of his opponents the significant taunt "Krauses Haar, krauser Sinn." To be *cross*, then, is to have a mind that is "curled, crooked." A similar turn of thought appears in the word "crook," and Lombroso and the anthropological psychiatrists may well be pardoned for maintaining that a "crook" is a man with a "crooked body" as well as a "crooked mind."

Zorn (anger, wrath, passion, rage, indignation, irritation), which in Middle and Old High German had the further meanings "quarrel, wordy encounter, brawl, dispute, violence, rage of the elements, affront, insult," is cognate with Old Saxon *torn* (anger, indignation); Dutch *toorn* (anger); *torn* (shock, strife, contest, tearing apart of a seam, ripping); Anglo-Saxon *torn* (anger, insult), and seems to be an old participle from the root *ter* seen in our verb *to tear*; Old High German *zēran* (to tear, destroy); Gothic *gatairan* (to tear); Russian *drate* (to tear); Lithuanian *dirti* (to flay); Greek *dēpeiv* (to flay); Zend *dar* (to cut); Sanskrit *dri* (to burst, to burst open, to tear asunder); the Indo-European radical at the base of all being *der* (to burst, to tear asunder). We find also the verb *ziernen* and the adjective *zornig*. Judged by the word *zorn*, therefore, "anger" reveals "a torn mind"—we still say "distracted with grief" and "torn by conflicting emotions," and speak of "tearing around," "being on a tear."

Another word for "anger, fury, rage" in Modern German is *Grimm*, an indication of whose older signification is found in the compound *Bauch-grimmen*. In Old High German *grim* meant "anger, rage, hostility, fierceness, pain;" Dutch *grim* (anger, fury). In our "*grim* Death," we have preserved one of the many meanings of the corresponding adjective (fierce, angry-looking, etc.); cog-

nate with Dutch *grimmig* ("angry"—*grimmen*, "to foam with rage"); Icelandic *grimmr* (grim, stern), Danish *grim* (grim, ugly); Swedish *grym* ("cruel, grim, furious"—*grymta*, "to grunt"); Old High German *grim*, *grimmi* (wild, fierce, hostile, terrible, violent, painful); Modern High German *grimm*, *grimmig* (enraged, furious, wrathful, fierce, violent, grim). Here again the kinship of "anger" and "sorrow" appears, for from the same root as *Grimm* comes *Gram* (grief, sorrow, etc.). The adjective *gram* (hostile) is cognate with Icelandic *gramr* (wrathful); Danish *gram* (wrathful); Gothic *gram* "angry"—seen only in the verb *gramjan* (to make angry, to excite to anger); Anglo-Saxon *gram*. The Anglo-Saxon *grimetan* (to rage, roar, grunt); Russian *gremiete* ("to thunder"—*grom*, "a loud noise"), and the distantly related *grin*, *groan*, *grumble*, indicate the ultimate origin which is from the Indo-European *ghrm* (to make a loud noise), derived from the more primitive *ghr* (to make a noise, to yell). In like manner we speak of a *grumpy* or *grumpish* man, meaning one who is crabbed or ill-tempered. Employing the same metaphor we speak of "howling with rage," "bellowing with anger," and "groaning in spirit."

A very common expression in German for "to be angry" is "auf einen böse sein." *Böse*, which now signifies "bad, evil, wicked, angry, sore, cross, ill-tempered, malicious," and of children, "naughty," is peculiarly a German, word not being found in other dialects. In Middle and Old High German *bæse* and *bōsi* had the meanings "bad, worthless, evil, greedy, slanderous," and Kluge cites the Old High German *bōsōn* (to slander, to revile) as indicating that the original meaning of *böse* was "slandering, maliciously speaking."

In Middle English we meet with *wodewroth* (madly angry) and *wode* (mad, raging), the *wood* (mad, furious) of Shakespeare, cognate with Icelandic *óðr* (raging, frantic); Gothic *wods* (raging, raging, possessed). The corresponding substantive is seen in Dutch *woede* (rage, fury, madness); Modern German *Wut* (rage, fury, madness; the adjective is *wütig*, the verb *wüten*). The Teutonic radical from which all of these come is *woda* (mad, furious, frantic). In Lowland Scotch *wod* or *wud* means "raging mad, stark mad." Related are also Anglo-Saxon *wōð* (voice, song); Icelandic *óðr* (poetry, song); Latin *vates* (bard, god-inspired poet); Irish *faíth* (bard), the radical idea being indicated by the Sanskrit *vat*—"to stir up the mind, to incite the mind"—a bard is one whose mind is filled with divine frenzy. Here belongs also perhaps the god *Woden*, whose *wütendes Heer* is well-known in German mythology. We even yet speak of a man in anger as being "stirred up," "aroused to indignation," "moved to wrath," etc.

Ire, *fury*, *rage*, *indignation*, *choler*, *passion*, *resentment*, we have borrowed from Latin, through French. *Resentment* (from French *ressentiment*, ultimately derived from Latin *re*, "again," *sentire*, "to feel," like the verb "to *resent*," has changed from its original signification, "being sensible of, having a sensible apprehension of," to that of being aggrieved at, taking ill, being indignant at, getting angry at." *Passion*, which in English means "suffering, strong agitation of mind, rage," comes to us through French *passion*, from the Latin *passio*, "suffering," cognate with *patior* (I suffer, endure).

Indignation, "anger at what is unworthy," is derived through French *indignation*, from Latin *indignatio*, "displeasure, indignation, disdain," which comes from *indignus* (I consider unworthy—*indignus*—I am indignant, I am displeased at). We find also Latin *indignitas* (unworthiness, indignity, indignation). In English

the phrase "righteous indignation" indicates the general idea at bottom of the word.

Ire, a word somewhat more elevated in stylistic use than *anger*, comes to us through French *ire*; from Latin *ira* (anger, wrath, passion, rage, violence, fury, indignation), of which the ultimate etymology is doubtful. A derivative of *ira* is *iracundia* (proneness to anger, hasty temper, irascibility, anger, wrath, rage, passion, violence). Familiar phrases are: *ira inflammatus*, *ira commotus*, *ira amantium* (lovers' quarrels). From its derivative, *irasci* (to become angry) is derived the adjective *irascibilis*, whence through French, our *irascible* (given to anger, choleric).

Fury, "anger, rage, passion," is derived through Old French *furie*; from Latin *furia* (fury, rage, madness, passion), cognate with *furere* (to rage, to be mad). Skeat correlates with Sanskrit *bhuranya*, "to be active," and refers back to the radical *bhur* (to move about quickly).

Rage, "fury, violent anger," comes into English through French *rage*; from Latin *rabies* (madness, rage, fury). In French *rage*, like the Modern Latin *rabies*, is applied to a mad dog — hydrophobia — and to other animals as well. The verb *rager* signifies in French "to be in a passion, to be angry, to sulk," and *rageur*, "a peevish person." Latin *rabies* is from *rabere* (to rave, to be mad); cognate with Sanskrit *rabh* (to desire vehemently, to act inconsiderately, to seize); the radical of both being Indo-European *rabh* (to seize). From a Low Latin word *rabiare*, derived from *rabia*, a by-form of *rabies*, come Spanish *rabiar* (to rave); Old French *resver*, French *rêver* (to dote, speak idly, rave), Old French *râver*, whence *ravasser* (to rave, to dote, to talk idly), and English *rave* ("to be mad, to talk like a madman;" we have also the phrase "raving mad").

Choler, "bile, anger," through Old French *cholere* (chola, anger); Latin *cholera* (bile, bilious, complaint, cholera), goes back to Greek *χολέρα* (cholera—from *χολή* [also *χόλος*], "gall, bile, rage, anger, wrath, bitterness, anything causing disgust or aversion). These Greek words are cognate with Latin *fel* (gall, bile, anger, rage, animosity, bitterness), and English *gall* (gall, bitterness, anger, bile). The physical basis of the idea is clearly the "bile, gall." From Latin *bilis* (bile, anger, wrath, choler, indignation); through French *bile*, comes our word *bile* (secretion from the liver, bitterness, anger, etc.). Here, again the physical basis of the idea is plain.

Skeat defines *spleen* as "a spongy gland above the kidney, supposed by the ancients to be the seat of anger and ill-humored melancholy," and we talk of "venting our *spleen* upon any one," and of a *splenetic* person—the word comes through Latin *splen* from Greek *σπλήν*, cognate with Sanskrit (*Splīhan*).

In Nipissing, a typical Algonkian dialect of Canada, the words for "anger," *nickatisiwin*, "to be angry," *nickatis*, etc., come from the radical *nick*, which signifies both "angry" and "gland," showing clearly the physical basis of the concept. To Nipissing correspond the Ojibwé *dishkadiiswin*, *nishkadis*, *nishk* (5, p. 270).

Canon Farrar says (6, p. 197): "In Greek the diaphragm (*φρήν*, *renes*, reins) is used for the understanding; the liver for feeling; the breast for courage; the nostrils for contempt (cf. *μνηστρες*, etc.); the stomach and the bile for anger. Similarly in Latin the nostrils are used for taste and refinement; the nose for satire; the eyebrow for sorrow or disdain; the stomach for anger; the throat for gluttony. The Lithuanians use the same word for soul, heart, and stomach, and the same is probably true of many nations. Many of these metaphors have been transferred to English, and we also use the

blood for passion (hot or young blood), the phlegm for dullness, the spleen for envy; we say that a person has sanguine hopes; we talk of a melancholy man, which means properly a man whose bile is black; a man has a nervous style, or is nervous in the hour of trial; and we say of a bitter-minded critic that he has too much gall."

We speak of "*ferce* anger," and even use *ferce* in the sense of "violent, angry, wrathful." The derivation of the word is through Old French *fiers*; from Latin *ferus* (wild, savage), cognate with *fera* (wild beast). Other phrases in use are "*wild* with rage," "*savage* resentment," etc. Here belong the comparisons: "*Mad* as a hornet," "*angry* as a bull," "*cross* as a bear," etc.

The same writer also says: "In Hebrew the heart, the liver, and the kidneys are used for the mind and understanding; the bowels mean mercy, like the Greek *σπλάγχνα*; 'the flesh' means lust; the loins strength; the nose is used for anger, so that 'long of nose' means patient, and 'short of nose' irritable; a 'man of lips' is a babbler (Job xi:2; the neck is the symbol of obstinacy; the head of superiority; thirst or paleness the picturesque representation of fear" (6, p. 196-7).

Shakespeare, in *1 Hen. VI*, iv, i, 141, makes the king say:

"How will their grudging *stomachs* be provoked
To wilful disobedience, and rebel?"

and in *Antony and Cleopatra*, iii, iv, 12, Octavia bids Antony:

"Believe not all, or if you must believe,
Stomach [i. e., resent] not all."

and in Elizabethan literature the word *stomach* had, as had *stomachus* in Classical Latin, the meanings "pride, courage, indignation, anger, resentment, ill-will." Hooker, in his *Eccelesiastical Polity*, says of Arius, that he "became through envy and *stomach* prone unto contradiction." The verb to *stomach* corresponds to French *s'estomaquer*, Latin *stomachari*.

Dr. Holder says: "The ancients made the spleen the seat of melancholy and other ills. Those people living in the malarial belt of the great Mississippi valley, with whom most of my life has been passed, charge to the liver all the ills from which flesh or mind may suffer, while the Indian declares me *spōr' kow-ekē*, 'my stomach is bad,' and is truly nearer the right" (10).

In Greek *kardia*, signifies "stomach," as well as "heart," just as *cœur* does in Modern French.

In the Kootenay Indian language of British Columbia the word for "angry" is *sāniltwīnē*, which signifies literally "bad-hearted he-is," from *sānē* (is bad), *iltwī* (heart, mind) — the opposite is *sūkiltwīnē*, "well-disposed, glad, happy," from *sūkinē* (is good), and *iltwī* (heart, mind). In analogous fashion are formed *sāniltqōnē*, "sick," literally "bad-bodied he-is," and *sūkiltqōnē*, "well, healthy," literally "good-bodied he-is" (4, p. 394).

According to Park, the African explorer, in the Mandingo, a language of Western Africa, the words "anger" and "angry" are expressed by *jusu bota*, literally "the heart (*jusu*) comes out" (12).

Of the Western Déné Indians of British Columbia, Father Morice remarks: "A single sentence, or periphrastic locution is all that the Carrier has at his disposal to give utterance to such varied movements as sorrow, melancholy, repentance, morosity, displeasure, etc. When moved by any of these, or cognate sentiments, he will never say but: *stzi ndācta*, 'my heart is sick.' The expression *utzi-sāstsi*, literally "his heart is acrid," signifies "he is acrimoniously disposed" (11, p. 207).

The primitive Aryans seem to have located in the heart and the viscera the seat of the life of man, the soul, and the emotions, and the languages of their descendants bear many traces of these ideas. We find in Latin: *cordatus* (wise, prudent), *vecors* (senseless, mad, insane), *recordari* (to recollect, call to mind), *credo* ("I believe")—from *crd* + *dh*), etc.; in German: *herzhaft* (dear, beloved), *herzlich* (cordial), *herzlos* (heartless, faint-hearted), etc.; in English: *heartly*, *heart-broken*, *dishearten*, *heart-rending*, *heart-whole*, *heart-felt*, *heartless*, *black-hearted*, etc.; in French: *sans cœur* (heartless), *au cœur dur* (hard-hearted, heartless), *de bon cœur* (heartily), *avoir le cœur fendu* (to be broken hearted), etc.

In Latin and Greek the liver (*jecur*, *ἥπαρ*) "was represented as the seat of the passions, especially of anger and love" (7, p. 265).

Of the Twaka Indians on the head-waters of the Princeapula river in the Mosquito Territory, Central America — although their neighbors the Mosquitos base their special vocabulary upon the word for heart (*kupia*), just as we do — Dr. D. G. Brinton tells us: "The Twaka Indians locate the seat of man's life and emotions, not in the heart, as most nations, but in the liver; and they have in common use such expressions as:

issing sawram, liver-split = angry;
issing pint, liver-white = kind;
issing sant, liver-black = unkind."

With these rude savages "kind" means "white-livered" and the gap between them and the cultured Englishman of to-day is somewhat lessened when we remember that in our own adjective *white-livered* (cowardly) we have preserved a memorial of that far-off past, in which the mind of primitive man failed to distinguish between "kind" and "cowardly." The English *white-livered* and the Twaka *issing-pini* lead us back in the history of mankind to a time when *kindness* to a foe was held to be *cowardice*.

Of the Terraba or Tiribi Indians of Costa Rica, Bishop Thiel is quoted by Dr. Gatschet as saying: "Many of the sensations and mental processes which we attribute to the heart are attributed by the Costaricans to the liver, *guo*, and hence such words as *to think*, *remember*, *forget*, *desire*, *sad*, *joyful* are compounded with the syllable *guo*" (8, p. 217).

In sixteenth-century English (the *Satires* of Bishop Hall) we meet with the expression "*liver-sick* of love" (sick at heart).

The Greek *θυμός* (spirit, courage, passion, anger, rage, wrath; soul, heart, life) is derived from *θίω* (I rush or dart along, storm, rage), and from the same root comes *θύάς*, "a mad or inspired woman, a Bacchant." The word is cognate with Latin *fūmus* (smoke), and in English we still speak of "*fuming* with anger," "to get into a *fume*." Sterne even uses *fume* in the sense of "a passionate person." We also use the expression "*storm* of passion," "*to storm*," "*a hurricane* of wrath," etc. *ὀργή* (impulse, passion, anger, wrath, violent emotion), together with the verb *ὀργάω* (I swell with lust, am excited, passionate—the word is also used of fruit in the sense "to swell as it ripens," of soil, "to swell with produce," etc.), is derived from the root *ὀργ* (to swell). We also say "*swollen* with anger."

In the language of the Samoan Islands *huhu*, the word for "anger, rage" signifies literally "*swell, swell* (*hu*=swell), as we say "*swelling*, *bursting* with passion" (12a).

μῆτις (wrath, anger, malice) comes from the root *men* (to be excited in thought, to be inspired, raving, wrathful, etc.), whence also *μῆτις* (might, strength, spirit, courage), *μανία* (madness), *μάντις* (a diviner, a seer) — at the basis of all these lies the idea of "mental

excitement." As Latin *mens* (mind) is cognate, all derive ultimately from the Indo-European radical *man*, "to think."

In the language of the Pacific island of Tahiti "*riri*," anger, literally means "*he shouts*" (13, p. 89).

In the Stikkeen dialect of the Tlingit language of Alaska we meet with the following expressions: *K'ant-wa nuk*, "angry," *K'anraō*, 'cross,' *K'ān-ga-gaō*, "I am angry." Here *wa nuk* and *raō* are verbal suffixes, between which, as in the last word *K'ān-ga-gaō*, and the radical *k'an* (angry) the pronoun is inserted. *K'ān* (angry) bears a suspicious resemblance to *K'ān* (fire) (2, p. 65). To this category belongs our "incensed." We speak also, as do other peoples, of "*kindling wrath*," and "*smouldering anger*."

A most interesting word in Greek is *νέμεσις* (righteous indignation, anger, wrath, resentment), personified in *Némeis*, the goddess of divine wrath and just retribution, cognate with *νέμωσις* (a distribution), from the verb *νέμω* (I distribute, possess, etc.), all from the Indo-European radical *nem* (to pasture, to number, to allot).

In his dictionary of the Niskwalli language of Washington, Dr. George Gibbs gives the following interesting etymologies: "*O-het-sil*, 'to be angry,' *o-het-sil-chid-hwul-dug-we*, 'I am angry with you,' from *o-het*, 'why, what is the matter?' and *st-lus*, 'the forehead.' Derivatives are *ōd-het-sil-us*, 'to sulk, to blush,' *o-he-ha-het-sil*, 'to pretend to be angry.' The radical *o-het-sil* signifies also "to be ashamed," *o-het-sil*, "to be angry," being distinguished from *o-het-sil*, "to be ashamed," only by intonation (9, pp. 309, 310, 348, 296, 351).

If we arrange the words for "anger" discussed above according to the ideas upon which they are based, we have the following:

1. Physical idea of "choking, strangling." English *anger* and its cognates.
2. Physical idea of "writhing, twisting." English *wrath*.
3. Idea of "crookedness, curling." English *cross* and its cognates.
4. Idea of "bursting, tearing asunder." German *Zorn* and its cognates.
5. Idea of "hasty movement." English *fury* and its cognates. Greek *θυμός*.
6. Idea of "seizing upon, grasping after." English *rage* and its cognates.
7. Idea of "making a noise, yelling." German *Grimm* and its cognates. Tahitian *riri*.
8. Idea of "malicious talk, slander." German *böse*.
9. Idea of "mental excitement, excitation." Latin *vates* and its cognates. Greek *μανία*.
10. Idea of "swelling." Greek *ὄργη*. Samoan *huhu*.
11. Based upon the "heart." Kootenay *sānilwinē*. Déné *stzi ndæta*. Mandingo *jusu bota*.
12. Based upon the "liver." Mosquito *issing sawram*. Térraba.
13. Based upon the "gall, bile," "spleen," "gland." English *choler*, *bile*, *gall*, *spleen*, and cognates. Latin *fel* and cognates. Nipissing *nickatisiwin*.
14. Based upon the "stomach." Latin *stomachus* and cognates.
15. Based upon the "nose." Hebrew.
16. Based upon the "forehead." Niskwalli *o-het-sil*.
17. Based upon the idea of "indignation at what is unworthy." Latin *indignatio*. Greek *Némeis*.

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A LABORATORY COURSE IN PHYSIOLOGICAL PSYCHOLOGY.

BY EDMUND C. SANFORD.

(*Fifth Paper.*)

THE VISUAL PERCEPTION OF SPACE.

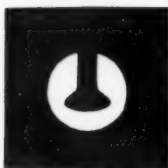
It is upon this field more than any other in physiological psychology, unless it be that of the psycho-physic law, that discussion has been most protracted and the accumulation of evidence most imposing. A complete treatment of the question involves arguments from surgery, pathology and other sources quite outside the possibilities of the laboratory. And even then, it is difficult, if not impossible, to establish one theory surely as against all others. Apart from the question of original sensations, however, there is a certain degree of harmony among investigators, and it is the commonly accepted experimental facts that this section of the Laboratory Course aims to gather up. The discussion of the ultimate matters may be followed in the works of Helmholtz, Hering, Stumpf, James, Wundt, and others. For the facts of spatial vision in general, see the works of Helmholtz, Hering, Aubert, Wundt, James and Le Conte. For special facts, special references will be given below. The subject is also more or less fully treated in the standard physiologies, Bernstein's *Five Senses*, McKendrick and Snodgrass's *Physiology of the Senses*, and other books of the same kind.

The ordinary seeing of space in its various aspects of distance, direction and size, rests on the retinal and kinæsthetic sensations of both eyes. And in every normal act of vision all of these sensations are either present themselves or by their reproduced images, and this fact must not be forgotten. For the sake of simplicity, however, it is necessary to separate the factors more or less completely, and to treat now of one and now of another. Those phenomena of which the presence of two eyes is an essential condition form a class by themselves, and will be reserved for a special section; the present one will be given to a portion of the facts of monocular vision, or rather to some cases in which the presence of a second eye is unimportant.

MONOCULAR PERCEPTION OF SPACE.

156. The Outward Reference of Visual Perceptions. The outward reference of visual perceptions probably comes about through their co-ordination with those of other senses, especially those of tactual and kinæsthetic origin, but the matter is too complex for direct experiment. It is easy, however, to study the relations of the retinal image and the outer objects that produce it, and much has been written on the outward projection of retinal states. It

must, however, be kept clearly in mind that retinal states as such, are never perceived, and especially that retinal sensations are not first given a location in the eye, and then at some later stage transferred outward. Considered physically, the retinal image is reversed with reference to the objects that it represents. This has already been seen for the rabbit's eye in Ex. 99, and for the human eye with Purkinje's vessel figures and the phosphenes (Exs. 107 and 113). It can be shown also in the following experiment with retinal shadows:



Le Cat's experiment. Hold a pin, head upward, as close as possible before the pupil, and an inch or two in front of the pin, a card pierced with a pin-hole. Move the pin about till it comes into exact line with the hole, when there will be seen in the circle of diffusion representing the hole a shadowy inverted image of the pin-head, somewhat as appears in the accompanying cut. The rays of light from the pin-hole are too divergent to be brought to a focus on the retina, but enter the eye in a favorable state for casting a shadow. The shadow on the retina is erect like the pin that casts it, but is perceived inverted in its outward location. Observe at the same time the still more blurred, erect image of the pin through which the other things are seen. This is not a shadow but a true retinal image formed in the ordinary way by light reflected from the surface of the pin. When several pin-holes are used (three at the points of an eighth of an inch triangle, for example) an equal number of shadows will be seen.

The casting of the shadow can easily be illustrated with a candle, a double convex lens and a bit of card. Set the lens a foot or two from the candle and hold the card too near to the lens for the formation of an image, then introduce a finger or pencil close before the lens on the side toward the light and observe the erect shadow on the card.

Le Conte, *B*; Wallenberg, Laqueur.¹

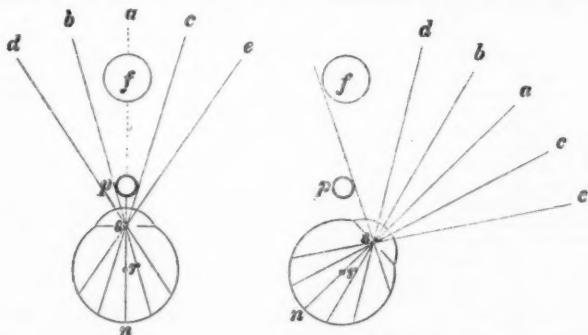
157. **Monocular perception of direction; directions from the eye.** The perception of direction is ordinarily binocular, and the centre to which directions are related lies between the eyes, even when one is closed. This will be proved in the experiments on binocular perception of space. The binocular perception, however, must rest on a perception of the relative direction of points in the monocular field, and this will be considered in the next few experiments.

Two luminous points appear to have the same direction when one is exactly covered by the other, or, to state the matter in retinal terms, when the image of the one for which the eye is accommodated lies in the centre of the circle of diffusion of the one for which the eye is not accommodated, or, if both appear in diffusion circles, when the centres of these circles coincide. The lines

¹See bibliography at the close of the article.

drawn through points in this relation and prolonged to the retina are known as *sighting lines* (*Visirlinien*, *Lignes de visée*), and cross in the centre of the pupil, or rather, in the centre of the image of the pupil formed by the cornea, about 0.6 mm. forward of the true position of the pupil and 3 mm. from the summit of the cornea. These lines might well be called "lines of direction," had not this name been already given to another set of lines, namely those which are drawn from the points of external objects to the corresponding points of their retinal images. These have already been mentioned in Exs. 101 and 112, and they give with certain limitations the directions in which objects appear when the eye is exactly accommodated for them. Their point of intersection is about 7 mm. from the summit of the cornea. They are important for optical purposes, but for the general perception of direction are less important than the sighting lines, though for remote points and for points near the fixation point, the differences between the two sets of lines is very slight. For points remote from the fixation point, for reasons to be given in a later experiment, neither set of lines gives the direction in which objects are seen.

The position of the crossing point of sighting lines is found by inference from the optical structure of the eye. To make a sure empirical determination would be laborious, but it is easy, however, to show that the crossing point is considerably in front of the centre of rotation of the eye (about 10.6 mm.). Place a candle at a distance of a foot or foot and a-half from the eye. Look toward the flame with a single eye, but hold close before the eye a pencil or narrow strip of black cardboard. So long as the eye looks straight forward, the flame is entirely hidden by the pencil. When, however, the eye is turned strongly to either side, the flame instantly appears on the side toward which the eye has been turned. The explanation of this will readily be seen from the following diagrams in which *p* represents the pencil, *f* the flame,



s the centre of sighting lines, and *r* the centre of rotation. The lines radiating from *s* are sighting lines, *sa* being the principle one, which is practically coincident with the line of sight.

Helmholtz, A, F, 002 (539), 745 ff. (584 ff.); Aubert, A, 461 (on the sighting lines).

158. Monocular perception of direction; directions in the field of vision. The relative direction of points in the field of vision

cannot be changed, of course, without changing the direction of the points from the eye; it is easier, however, to experiment on points in the field.

a. Lines that appear straight in indirect vision. Lay a large sheet of paper on the table and mark a fixation point in the middle of it. Two or three inches to one side of the fixation point place a button or bit of black paper, and, a foot above and below, other buttons or bits of paper. Then leaning over the table so as to bring the eye above the fixation point try to place the three buttons in a straight line, holding the eye steadily upon the fixation mark. Examination of the buttons when placed will show that the middle one is too near the fixation mark, i. e., the attempt to make a straight line has resulted in a curve convex toward the fixation mark. Try also with the buttons in a horizontal line.

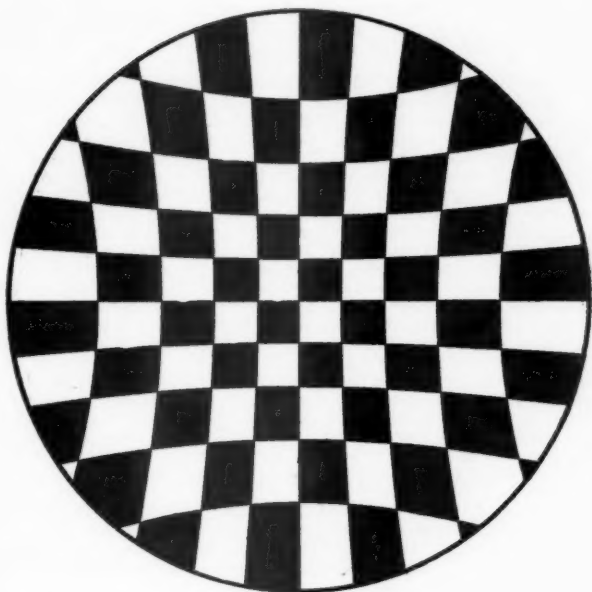
b. Actual straight lines in the periphery of the field. If lines convex toward the fixation point appear straight, lines that are actually straight should appear concave. On a large sheet of paper draw a pair of parallel lines three or four inches apart and two or three feet long. Place a fixation point half way of their length and half way between them; fasten the paper to the wall or spread it on the table, and observe as in a above. Try with the lines both vertical and horizontal and in oblique positions. The same thing in part, at least, may be noticed in the double images of a single vertical thread seen binocularly.

In a spherical field of vision, the parallel lines of experiment b would be represented by great circles, the horizontal pair for example, having their poles at the right and left ends of the horizontal axis of the field, their planes making equal angles above and below the plane of the horizon.¹

It is obvious that changes in direction which make straight lines appear curved cannot take place without introducing slight errors of distance also. The shortest distances for perception are the curves which appear straight.

c. Nature of lines that appear straight in indirect vision. It would, of course, be possible by developing the method used in a above to make a somewhat exact study of the nature of the lines that appear straight in indirect vision, but their general nature may be found in another way. In the hemispherical field of regard these lines are circles — Helmholtz's *Circles of Direction*. The following diagram shows the projection on the visual field of a system of these circles of direction. For use the diagram must be enlarged five or six times. It should be viewed with the single eye opposite its centre, and at a distance proportionate to the length of the short line below the diagram. In order to fix this distance, it is convenient to cut a small rod of such length, that when the eye is at the right distance the rod will just extend from the outer edge of the socket of the eye to the diagram. When the head is brought into the proper position and the eye is fixed on the middle of the diagram, the lines of the figure will appear approximately straight and parallel. Try with the diagram in the position shown below, and also when turned so as to make the principle lines oblique. Especial care should be taken to avoid movements of the eyes, for a new interpretation of the curves is thus introduced, and the checker-board seems concave instead of plane. Some dis-

¹It should not be supposed from this that the naïve field of vision is hemispherical. It is neither definitely hemispherical nor definitely anything else, except as it is formed by the conditions and habits of vision. It is spoken of as a plane or as a hemisphere for greater ease in exposition.



advantages are escaped by fixating the centre of the diagram till a sharp and strong after-image is secured, and then observing this with closed eyes turned toward the sky.

After getting the general effect, the observer should repeat the observation beginning first at a distance (greater than that just used) at which the direction of curvature in the lines can easily be recognized, and then slowly decreasing the distance till a point is reached where the lines seem straight and the squares equal, and still further till the curves appear to bend the other way. Test the distance at which the lines seem straight with the little rod mentioned above; it will generally be found to agree approximately with the distance for which the diagram is calculated.¹ The projections of the circles of direction are then the lines that seem straight in indirect vision. These circles of direction are lines along which the eye (when moving according to Listing's Law) can carry a short after-image without causing the image to leave the line, in other words they are, for the eye in motion, straight lines; and the experiment shows that even when the eye is kept still, its

¹The agreement is not absolutely perfect, and there are perhaps, in addition, individual differences depending on the exactness with which the eyes follow Listing's Law. Helmholtz finds the curvature of the extreme verticals on the temporal side a little too great, and Küster working by a slightly different method appears to have found it too great for all the curves. (See Hering, *A.* 370, foot note.)

experiences of movement exercise a controlling influence on its perceptions. (For a fuller account of Listing's Law and the circles of direction, see Ex. 123, and the Appendix at the end of this section.)

d. Illusions of form in indirect vision. Radial distances, as the diagram of c shows, are more decidedly under-estimated than distances parallel to the margin of the field. This is easily shown by laying a large sheet of paper on the floor—a strip three feet long and a foot wide answers well, when the narrow side is next the observer—and standing so that when the eyes are directed horizontally forward, the paper will be seen at the lower edge of the field. When the eyes are turned from the horizon to the paper, the latter noticeably increases in width (*i. e.*, in a direction to and from the observer) when the eyes are again directed to the horizon, it suffers a corresponding contraction. Changes in the other direction are hardly noticeable. Disks of paper (three to six inches in diameter) when viewed in indirect vision appear as ellipses with their short axes directed toward the fixation point. For the part of the field near the fixation point such illusions as this and those of *a*, *b* and *c* are so slight that they may be neglected.

The whole field of vision itself appears narrower than it really is; it actually covers an extent of nearly 180° , and yet under favorable circumstances, as when looking at the dark field of the closed eyes, or at the sky in the absence of all landmarks, the extent may seem not much over 90° .

Helmholtz, *A. F.* 706-718, (551-561). Wundt, *A.* 3te Aufl., II, 112-114; 4te Aufl., II, 128-130. Hering, *A.* 300 ff., 535 ff.

159. Monocular perception of direction; directions in the field of regard. The observation that the perceptions of the eye at rest are modified by those of the eye in motion, is still further confirmed by the similarity of other phenomena of the field of regard and the field of vision.

a. Straight lines viewed with the eyes in secondary positions. Experiment with a single eye and a long ruler held horizontally before an even wall space or other uniform background. Hold the flat side of the ruler toward the face and about a foot distant from it. Try first with the ruler eight or ten inches above the primary position of the line of sight (*cf.* Ex. 123), running the eye freely back and forth along the edge, and observe that the edge appears curved upward, *i. e.*, concave below. Try with the ruler depressed a somewhat greater distance below the primary position and observe the contrary curvature. Try also with the ruler vertical and to the right and left. Little advantage will result from too extreme positions of the ruler. The curvature to be observed is not very great, but that it is due to the visual apparatus and not to the ruler, is easy to show by turning the ruler over, which would reverse the direction of curvature in the ruler, but not that of the curvature which depends on the eye. Change of position of the ruler from above to below the primary position of the eye, on the contrary, reverses the direction of the curvature due to the eye, but not a real curvature of the ruler. Compare the results here found with those in Ex. 158, *a* and *b*.

The occasion of the illusion is the rotation of the eyes when moved from point to point in secondary positions. (*Cf.* Ex. 123, and the Appendix at the end of this section.) When the eye is kept fixed on the end of the ruler, or moved slowly, the ruler may seem slightly tilted instead of curved.

b. The apparent vertical. For the single eye a true vertical appears to incline a very little inward, *i. e.*, to the left for the right

eye and to the right for the left eye. Place on the field of the cam-pimeter a large sheet of paper, and on it draw an exactly horizontal line at about the height of the eye when the observer is in position. Exactly above the point of this line to which the line of sight is naturally directed, set a tack and hang from it a black thread 60 cm. long with a weight at its lower end. At a distance of 57.3 cm. from the tack, draw a second horizontal line and paste along below it a bit of millimeter paper. Let the observer take his position and carefully push the weight to one side or the other (as may easily be done with a needle mounted in a short handle of wood) until the thread seems to be exactly vertical, and the angles that it makes with the horizontal line exactly right angles; then, let him hold the thread in place by sticking the needle into the wood and note the amount of the angle from the millimeter scale—on which, if the dimensions above have been observed, 5 mm. will correspond to 1° . In this experiment the eye may be moved up and down the thread as desired.

Repeat the experiment, keeping the eye fixed in the primary position. The amount of the inclination necessary has been found by Donders to be decidedly variable even in the same observer. The attachment of the eye muscles is such that with elevation of the lines of sight there is a slight turning outward, and with depression a slight turning inward. A line which the eye follows exactly in this upward and downward movement, i. e., a line inclined a little outward seems vertical.

On a see Helmholtz, A, F, 609 (545); Hering, A, 538. On b see Helmholtz, A, F, 700 f. (546 ff.); 716 f. (550 f.); Wundt, A, *Sie Aufl.*, II, 122 ff.; 4te Aufl., II, 140 ff.

160. The tendency of the eye to follow lines and especially contours. This tendency is of importance in the seeing of form, because it results in clear vision of the object and because it complicates the whole matter by introducing kinæsthetic factors. It is a habit, however, that is not beyond conscious control, and for that reason is more difficult to demonstrate by overt experimentation than by casual observation. Any one that will take note of his own seeing when presented with objects with strongly marked lines, will easily find trace of the habit. In imagining geometrical figures, also (for example, an eighteen-inch hexagon drawn on the blackboard) something of the same tendency will often be found. The following experiment aims to give a laboratory means for making such observations.

Paste upon a piece of cardboard eight and one-eighth inches long and four inches wide, two four-inch squares of red paper in such a way as to cover all the card, except a white stripe one-eighth of an inch wide between them; cover the whole with a sheet of semi-transparent paper as for Meyer's experiment (Ex. 142 c). Examine the white stripe for the effects of contrast. After the examination has lasted a few seconds, suddenly lay across the middle of the diagram a bit of wire six or eight inches long, approximately at right angles to the white stripe. If the experiment succeeds, the white stripe will instantly show a strong increase in the complementary color. Before the introduction of the wire, the eye is chiefly engaged in following up and down the white stripe, and the contrast effects are confined to those of simultaneous contrast. When the wire appears, the eye changes to it and moves back and forth along it once or twice, and thus brings upon the white stripe the more powerful effects of successive contrast.¹

¹This experiment originates with Waller (*Journal of Physiology*, XII, 4, p. xliiv), but is used by him for a totally different purpose.

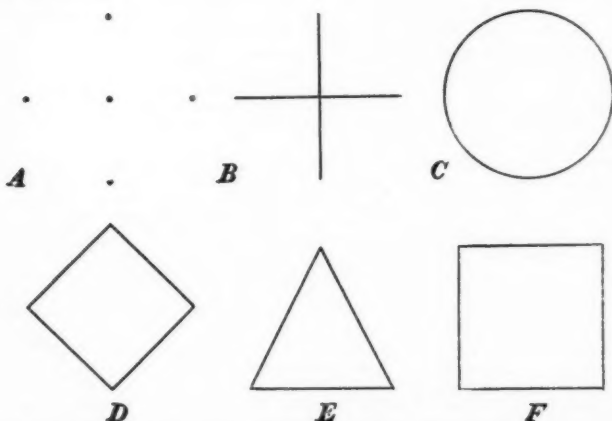
GEOMETRICAL ILLUSIONS.

Besides the illusions just considered, there are a large number of others that show in greater or less degree the influence of eye motions in the formation of visual perceptions. It is very likely that many of them — even those that appear simple — are the resultant of many other experiences than those of ocular motion, but in most of those that are to be given here, eye motions are certainly an important factor. They fall into classes according to certain empirical principles and have been so grouped below, but merely for convenience and without any intention of prejudicing their explanation. Typical examples only are given, for the number of variants of some of them is very large.

In all of them the student will do well to turn the diagrams about and to view them from different sides so as to separate the illusions that depend on position from those that are independent of it. In general, illusions are strengthened when the affected lines are made oblique in the field, corresponding with the less frequency and certainty of eye movements in such directions. For the most careful study of these illusions they should be separated from one another, and from the influence of all extraneous lines, *e. g.*, drawn singly on good sized sheets of paper.

161. Illusions in the perception of distances depending on their direction in the field of vision.

Vertical distances are over-estimated in comparison with horizontal distances. Lay off by eye on a sheet of paper placed vertically before the face equal distances, up, down, right and left, from a central dot, marking the distances by dots as in *A* in the accompanying diagram. Repeat several times and measure the distances found. In the diagram all the vertical and horizontal distances are equal, and in all cases, except in the circle, the vertical seems too long.



In *D* the over-estimation of the height entails an under-estimation of the angles above and below, and an over-estimation of those at the right and the left. The illusion is less in the line

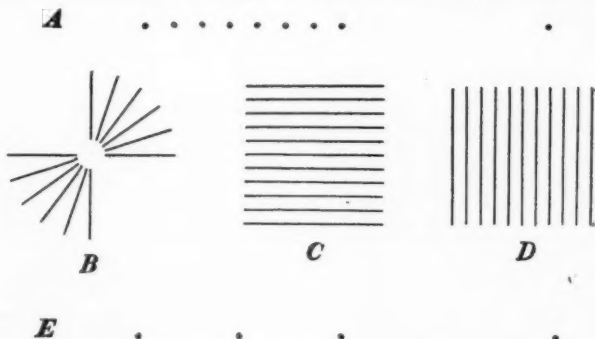
figures and absent entirely in the circle, in Wundt's opinion (4te Aufl. II, 152), because in the case of these familiar figures, perception is influenced by knowledge of the geometrical relations of the parts. (For quantitative studies see among others, Kundt, and Münsterberg, 164 f., 175 ff.) A similar, though slight, difference is often found between horizontal distances to the right and left, when careful experiments are made with the single eye. (For quantitative measurements under various conditions, see Kundt, and Münsterberg, 160 ff.)

Distances in the upper part of the field are over-estimated as compared with those below them. This illusion may be tested actively as follows: Near the top of a strip of paper twelve or fifteen inches long and five or six wide, draw a horizontal line two inches long. Take this again as a standard and draw half an inch below it a second line of a length that seems equal to the first. Then cover the first line and taking the second as a standard, draw a third and so on, continuing this process till the strip is full. Then uncover and measure the first line and the last. With this Wundt associates the S's and 8's which seem a little smaller at the top than at the bottom when in their usual position, but a good deal larger above when inverted: S S.

For all of these illusions Wundt finds an explanation in the differences of effort required for turning the eye in different directions (3te Aufl., II, 119 ff.; 4te Aufl., II, 137 ff.). The superior and inferior recti are relatively weaker than the external and internal muscles of the eye. Furthermore in elevating or depressing the eyes, the oblique muscles partly oppose the superior and inferior straight muscles, and so render a portion of the effort ineffective for purposes of motion. Why lines in the upper part of the field should seem longer than those lower down, is not specifically stated, but by implication it also is to be credited to muscular differences.

For references see those given at the end of Ex. 167.

162. Illusions of interrupted extent. Interrupted intervals



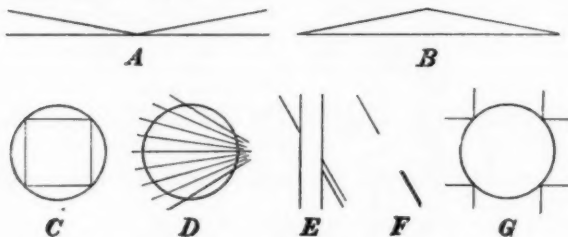
generally seem larger than free intervals. In the first two figures above, the interrupted extents seem larger than the free extents. In the last case, however, where the interrupted space has but a single dot in the middle, the principle suffers an exception. The

figure showing the filled angles should be regarded binocularly; monocular observation complicates the effect by introducing the illusion of Ex. 159, *b*. *C* and *D* are equal squares.

Here as before Wundt's explanation rests on variation in eye movements (3te Aufl., II, 126 f.; 4te Aufl., II, 144 f.). In passing over interrupted extents, the movement of the eye is made more difficult by the short stages into which its course tends to be broken, while it sweeps with relative freedom over the empty spaces. The fact that a single interruption in the middle of a space has an opposite effect, is explained by a tendency of the eye, when the middle of an extent is marked, to take in the extent simultaneously by fixation of the middle without motion. For other explanations see Helmholtz, *A*, F, 720 f. (563 f.).

For references see those given at the end of Ex. 167.

163. Illusions affecting the apparent size of angles. Small angles are relatively over-estimated. The figure with filled angles in Ex. 162 above might be classed under this principle as well as under that of interrupted extent, but in other figures the effect of small angles is seen by itself.



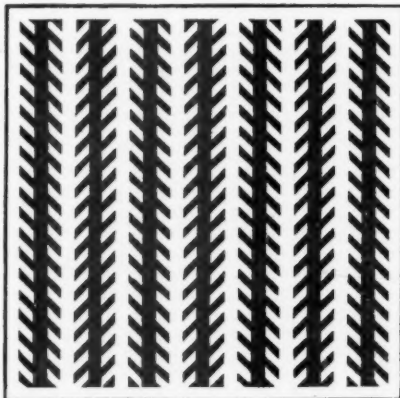
In *A* and *B* slight distortions are found in the horizontal lines. In *C* the circle is flattened at the corners of the square, and the sides of the latter are bent inward. In *D*, also, the distorting effects of the cross lines are unmistakable. In *E* the oblique line on the left is the real continuation of the lower line at the right, not of the upper as appears to be the case. This illusion is strengthened by viewing it from a distance, *i. e.*, by reducing the size of its retinal image. In *F* it is shown that the presence of an actual oblong is not essential to the illusion, though here, as elsewhere, the place of actual lines may be supplied more or less consciously by imaginary ones, or by the lines of other figures, the edges of the page or any other prominent lines in the field. In *G*, however, the explanation by the over-estimation of small angles completely fails, for the effect is the same in kind as in *C*, when the reverse was to be expected.

The over-estimation of acute angles Wundt refers also to eye movements (4te Aufl., II, 146). In figure *A* above, for example, as the eye follows the horizontal line toward its intersection with the oblique lines, it suffers an increasing attraction, as it were, toward the oblique line nearest it, and from this results the wrong conception of its route. The fifth figure according to Wundt involves several illusions. The figure is estimated too large in the direction of its prominent (vertical) lines, and in this the habitual over-estimation of verticals helps. When the latter is

excluded by turning the figure on its side, the illusion that remains is to be explained partly by the over-estimation in the direction of the prominent lines and partly by over-estimation of the small angles. In explanation of the first three figures, Helmholtz again cites his principle "according to which acute angles, being small magnitudes clearly limited, seem in general relatively too large, when we compare them with undivided obtuse or right angles," but believes that this principle yields in importance to ocular movements, if indeed, it does not itself depend on such movements (*F*, 724, 725, 726, 727, (566, 567, 568)). Cf. illusions depending on motions of the eyes, Ex. 163. In the fifth figure he thinks movement is less important, and that irradiation may co-operate (*F*, 723, (565)). The over-estimation of small angles is rejected as a principle of explanation by Lipps.

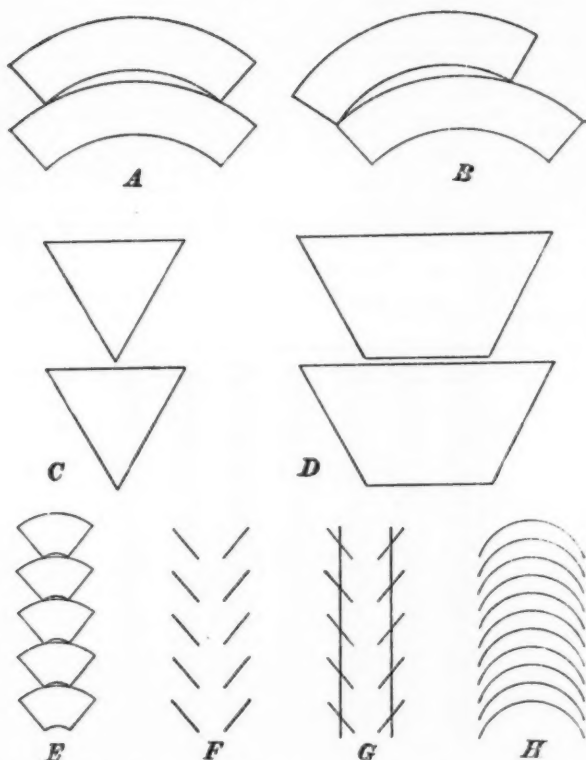
For references see those given at the end of Ex. 167.

164. Zöllner's figure. In principle this much discussed figure is the same as those of Ex. 163.



The oblique cross lines, making small angles with the verticals, cause an apparent dislocation of them in the same direction that they would be dislocated if the acute angle were enlarged. The greater effect in this figure would then be due to the multiplication of the small angles. For a partially concurrent explanation see Ex. 168; for divergent explanations see among others, Hering, Kundt, Lipps. The strength of the illusion depends on the angle of the oblique lines. For himself Zöllner found it greatest at about 30° . The short cross lines themselves show the illusion of *E* in Ex. 163, the right and left portions seeming not to be continuous.

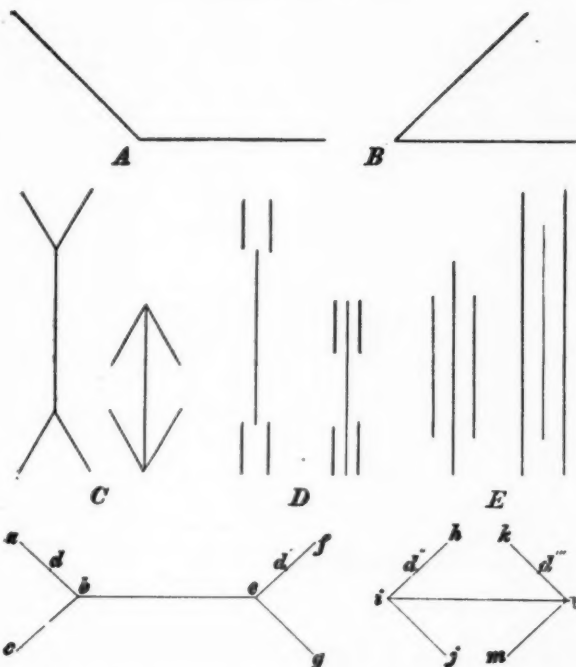
An interesting case in which Zöllner's figure is present, though masked, is that of the superposed segments of a ring. In *A* and *B* below, the upper one, in each case, seems smaller, though all are of precisely the same size. Müller-Lyer recognizes this as a case of the Zöllner's figure and suggests a transition to it by such figures as *E*, *F* and *G*. Wundt denies this explanation (4te Aufl., II, 151-152), asserting that if this were the case, the effect ought to



be the reverse, *i. e.*, the upper segment should seem larger, and gives *H* in which the upper curves seem a little larger in proof, but he evidently has only considered the curves and not the straight line at the ends of the segments. His own explanation which traces the illusion to association with cases in which the segments are referred to the same centre is clearly imperfect, for it does not fit the cases of the triangles and trapezoids, *C* and *D* above, where the illusion though weakened is still present. The fact may be that the ring segments involve co-operating illusions depending on both principles, while the straight line figures involve only one. The illusion is even more striking when the segments are cut out of cardboard, and can be shifted about and actually superposed.

For references see those given at the end of Ex. 167.

165. Illusions affecting the length of lines in the presence of other lines and angles. In *A* and *B* the sides of the larger angle seem



longer than those of the smaller angle. Try with the diagram in different positions to avoid the tendency to over-estimate vertical distances noticed in Ex. 161 above. In *C* the central vertical lines are of equal length, but that in the longer figure appears distinctly longer. In *D* and *E* the same is true, though the illusion is somewhat weakened.

A number of explanations for these illusions have been advanced, no one of which appears so exclusively right as to exclude the rest. (1) It has been held that figure *C* is only a development of *A* and *B* and that the angles modify the apparent length of their enclosing lines (Müller-Lyer, Jastrow). (2) It has been suggested that we unconsciously take into account the spaces adjacent to the lines to be compared; e. g., the trapezoidal spaces *a b e f* and *h i k l* inclined by the outwardly and inwardly inclined arms in the typical figure (Müller-Lyer and Auerbach). (3) Another writer considers that the tendency to over-estimate small angles and under-estimate large ones causes an illusory rotation of the short arms about their middle points, and thus a lengthening of the central line in one case and a shortening in the other. If, for example, *a b* and *e f*, were rotated in the sense required by this illusion about *d* and *d'*, their ends *b* and *e* would be brought farther apart. Similarly, but with opposite effect, if *g h* and *k l* were rotated about

their middle points (Brentano). (4) Others have thought that the eye in moving along the central lines tends to follow out upon the short lines when they are outwardly directed, and to stop short of the end of the central line when they are inwardly directed (Delboeuf, Wundt, 4te Aufl., II, 149 f.), and in a way Lipps.¹ This explanation is supported as against (3) by the persistence of the illusion in such figures as *D* and *E*, where there are no actual small angles. In *E*, however, they may well be imagined. (5) It has been held again that these figures are in reality like *D*, *E*, *F* and *G*, Ex. 167, and that we do not really estimate the distance from the apex of the short lines, but from the centre of one group of lines to the centre of the other, from the centre of group *a b b c* to the centre of the group *fe eg*, each perhaps being regarded as a triangle with an imaginary side (Brunot). The skillful observer will very likely find several of these tendencies appearing in his own study of the figure, and the explanations are not totally exclusive of one another. If the over-estimation of small angles depends as Helmholtz thinks is possible (*F*, 730, (571)) on eye-movements, the third and fourth explanations might find a common ground. Perhaps, also, the tendency to estimate the distances from the middle of the short line groups, has its effect by influencing the movement, as might also the tendency to take into account the adjacent areas, though less obviously. In any case, the motor factor is an important one.

166. Illusions showing the effect of adjacent extents.² In cases *A*, *B*, *C*, *D* and *E*, the extent of the middle quadrilateral, angle or line seems smaller when it lies between large extents than when it lies between small extents. Müller-Lyer seems to hold that the observer first compares the extents in question with their surroundings and then with each other.³

$\begin{array}{ccccccc} \cdot & & \cdot & & \cdot & & \cdot & & \cdot & & \cdot & & \cdot \\ a & & b & & c & & d & & e & & f & & g & & h \end{array}$

This process might be expressed schematically somewhat as follows :

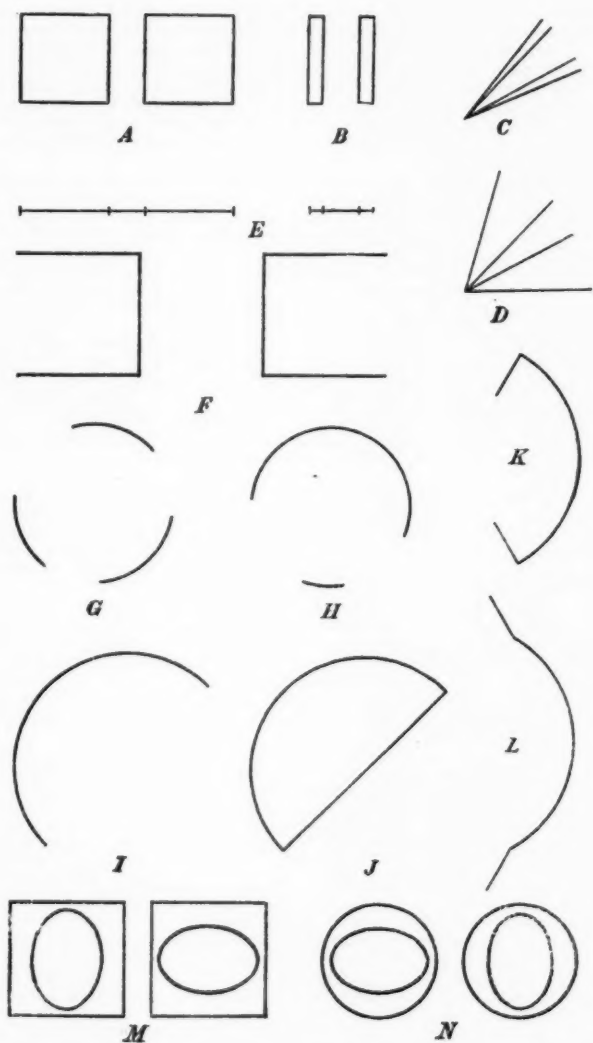
$\begin{array}{llll} bc & \text{in comparison with} & ab & \text{or } cd \text{ is small.} \\ fg & \text{"} & \text{"} & \text{"} \\ bc & \text{"} & \text{"} & \text{"} \end{array}$

In a similar way the sides and bases of parallelograms are said to be mutually affected, and even parallel lines seem nearer together or further apart as they are long or short. In this case, which is the only one noticed by him, Wundt (4te Aufl., II, 146-147) explains the illusion by the strong tendency to move the eyes lengthwise along the long parallels, which results in an under-estimation of the figure in a vertical direction. In figure *M*, the squares are distorted horizontally and vertically by the inscribed ellipses. In *N*, however, the circles are distorted, if at all, in an opposite

¹Lipps in his explanations makes use of many picturesque expressions with reference to these figures and their parts such, as, liveliness, inner activity, upward-striving (*Lebendigkeit, innere Regsamkeit, Emporstrebens*), thus seeming to attribute different effects to "forces" inherent in the figures, and for this he is criticised by Wundt. It seems probable to the writer, however, that if he had undertaken an analysis of the "forces" that he finds, he could hardly have avoided agreement with the eye-movement party. Indeed in his *Grundrissen des Seelenlebens*, p. 327, he makes use of eye-movements to explain illusions of the Zöllner type, though he regards them in the last analysis as psycho and not sensory illusions.

²To these illusions, Müller-Lyer gives the name of Illusion of Confusion (*Confusionsäusungen*).

³Müller-Lyer, 266-267.



direction. In *M* we have a case of the influence of adjacent lines ; in *N*, however, this effect is overcome by the greater power of the

illusion of Ex. 163, which here affects the diverging lines of the circle and ellipse. It seems possible also that we may have here at the points where the circles and ellipses are nearest together a compressing effect like that in the concentric circles in Ex. 167.

When the circumference of a circle is interrupted, the remaining arcs seem too flat to belong to a circle of such radius; so also, a semi-circle seems like an arc of a greater circle of less than 180° extent. Closing it by drawing the diameter makes it seem smaller. Cf. Figs. *G*, *H*, *I* and *J*.

This illusion Müller-Lyer connects with that noticed in Ex. 165, making a transition through figures *K* and *L*, the middle line being curved instead of straight. The short straight lines tend to lengthen and flatten one arc and shorten and bend the other. If short arcs of the same radius as the central ones be substituted for the short straight lines, and then be rotated till they form a continuation of the middle arcs, they might still be expected to produce their usual effect, from which would result the generalization that every portion of a circular arc influences the form of every other portion, in the direction of contraction in the complete circle and large arcs, and of expansion in the case of arcs less than 180° . Wundt, also, refers the case of the semi-circles to the same general principle as that of Ex. 165 (though his principle is somewhat different from Müller-Lyer's), but believes that the small arcs in *G* and *H* seem flat, because the movement of the eye in following them is not very remote from that for following a straight line (4te Aufl., II, 149 f., 151 f.).

When one side of a quadrilateral figure is removed, the figure seems too long in the direction toward the side removed and too short in the other direction. In figure *F* the three-sided squares seem too long in a horizontal direction, too short vertically. The reverse is the case with the space between them which is also a square of equal size.

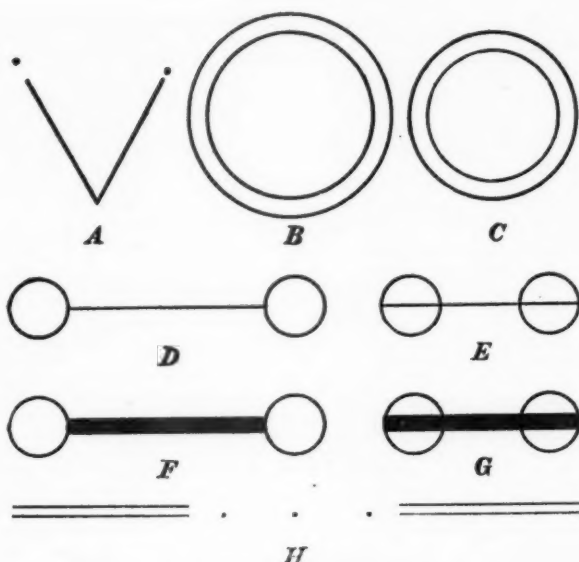
The explanation given by Müller-Lyer for this illusion is double. The open-sided square seems too long in the direction of the open side, because a certain portion of the free space is included in the estimate, and then it seems too narrow because it seems too tall.

167. Some unclassified geometrical illusions. An interesting figure from Łaska is given as *A* below. The sides of the angle are equal, but are made to seem unequal by the setting of dots at unequal distances from them.

In *B* and *C* is shown an illusion from Delbœuf. The inner circle in *B* and the outer one in *C* are the same size, but that in *B* looks larger and that in *C* smaller.

In Delbœuf's opinion, the illusion depends on the interference of the extra circles with the measurement of the diameter by the eye. In *C* the inner circle holds the eye back, as it were, and in *B* the outer circle draws it on. Wundt's explanation (4te Aufl., II, 146-147), is here, as in the case of the parallel lines already mentioned, the under-estimation of distances in directions opposed to the chief tendency to movement. In this case the eyes tend to follow the parallel circumferences which causes under-estimation of the distance between them, making the larger seem too small and the smaller too large. If this tendency to movement is opposed by a fixation mark in the middle, he finds that the illusion disappears, as in the case of the space broken by a single central dot in Ex. 162.

In *D* and *E* is shown another striking illusion, again from Delbœuf. The distance between the adjacent edges of the left hand pair of circles is the same as that between the remote edges of the right hand pair, though the latter looks considerably less. The tendency



apparently is to estimate the distance not between the points mentioned, but between the centres of the circles in each case. In *F* and *G* where the heavy lines interfere with this tendency, the illusion is a little weakened. For these figures, Delbœuf's explanation is like that for *B* and *C* above; in *E* and *G* the eye is restrained, in *D* and *F* it is drawn onward.

In *H* is shown an illusion from Mellinghoff. The three dots in the free space between the parallel pairs seem a little above the level of the lower lines of the parallel, though they are not actually so. The upper lines of the parallel pairs, according to Wundt (4te Aufl., II, 146), attract the eye as it is swept across the figure, toward a position intermediate between the upper and the lower lines, and to this position the dots are assigned.

In the parallel columns below is shown another illusion of common experience with printers :

In these two columns the type is of exactly the same size. On this side, however, it is set "solid" and looks smaller than on the other. According to Wundt (4te Aufl., II, 150), this is because the eye passes over the same number of letters in a shorter course.

Here the lines are "lead," i. e., have greater space between them. Is it not possible that the illusion is based on the greater general whiteness in this case and blackness in the other ?

For a collection and discussion of a number of other illusions, some similar to those of preceding experiments and some different, see Lipps, *A* and *B*.

On the Geometrical Illusions in general see Wundt, *A*, 3te Aufl., II, 115-132, and 4te Aufl., II, 137-156. Helmholtz, *A*, F, 720-733 (563-573).

On Ex. 161: Kundt, Münsterberg, 164 f., 175 ff. Wundt, *A*, 3te Aufl., II, 119 ff.; 4te Aufl., II, 137 ff. Helmholtz, *A*, F, 697 (544).

On Ex. 162: Wundt, *A*, 3te Aufl., II, 124 f.; 4te Aufl., II, 142 f. Helmholtz, *A*, F, 720 f. (563 f.). Knox. Watanabe.

On Ex. 163: Wundt, 3te Aufl., II, 125 ff.; 4te Aufl., II, 145 ff. Helmholtz, *A*, F, 722 ff. (564 ff.). Lipps, *B*, 299, 301. Jastrow, *B*, Dresslar.

On Ex. 164: Helmholtz, *A*, F, 722-725 (564-568). Wundt, 3te Aufl., II, 125 ff.; 4te Aufl., II, 144 ff. Zöllner, *A* and *B*. Lipps, *B*, 297 ff. Jastrow, *A*, and the literature cited by him.

On Ex. 165: Müller-Lyer. Jastrow, *A*. Brentano. Lipps, *A*. Delbœuf, *C*. Wundt, *A*, 4te Aufl., II, 149 f. Brunot.

On Ex. 166: Müller-Lyer. Wundt, *A*, 4te Aufl., II, 149 f., 151 f.

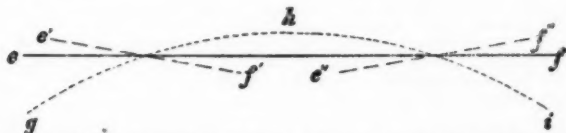
On Ex. 167: Láská. Delbœuf, *A* and *B*. Wundt, *A*, 4te Aufl., II, 146, 147, 150. Brunot.

168. Illusions depending directly on movements of the eye.

a. Move a pin head along the imaginary line *CD* in the figure below, keeping the eye constantly fixed on the pin head as it moves. The line *AB* will seem to move downward and to the left as the pin head goes from *D* to *C*, and upward and to the right as it goes from *C* to *D*. Steady fixation of the pin head is essential; a moderate rate of movement, which can be found by a few trials, gives the best result. The right and left movement of *AB* may be increased by moving the pin head in a line more nearly horizontal than *CD*. In this case movement of the image of *AB* on the retina is interpreted as movement of the line instead of the eye. For the fuller consideration of such cases see experiments on the perception of motion to follow.



b. An illusion affecting the direction of a line is to be observed when a compass point is made to draw an imaginary arc, cutting the line *ef* as the dotted arc *ghi* does in the figure below. As the point advances from *g* to *h* the line appears to take a position like that of *e'f'*; as the point traverses the region about *h* there is a sudden change, the line inclining now in the direction of *e''f''*. As before, constant fixation of the moving point is essential.



c. Something resembling a above is to be observed when the eye follows the movement of a pin head moved to and fro across the Zöllner figure in Ex. 164. As the pin goes from left to right the first, third, fifth and seventh black bands move downward, the second, fourth and sixth move upward. When the pin goes from right to left, the movement of the bands is reversed. The apparent inclination of the bands is also increased by the movement and the ordinary illusion intensified. The upward moving bands incline toward the side from which the pin starts, the downward moving incline in the opposite way. Moving the pin in a line parallel to the bands, decreases or abolishes the ordinary illusion. The illusion

of motion in the bands is evidently suggested by an illusory movement of the short oblique lines induced by the moving pin head in exactly the same way as the movement of the line $A B$ in *a* above. Constancy of fixation is important here as before; some observers may find this easier to accomplish if the pin is held still and the diagram moved behind it. A certain moderate rate of movement which may easily be found by trial is best. The writer finds some help from bringing the diagram rather near the eye, i. e., within six or eight inches.

The apparent movement so strongly present in this experiment is regarded by Helmholtz as the key to the explanation of the Zöllner and similar illusions. That such an illusory motion might be a factor is strongly suggested by the "unsteadiness" of the figure on casual examination. (See also *d* below).

d. Near the edge of the table drive a couple of small nails about two feet apart and lay a sheet of paper between them. On the paper and against the nails on the side away from the edge of the table lay a meter-stick or other long ruler. Take a pair of compasses of large span, say a foot; set one point close to the rule at one side and bring the other point down to the rule about the middle of the paper. Short arcs drawn with the compasses in this position would not differ much from perpendiculars to the rule. Bring the compass point to a division of the scale, fixate the division mark and then move the compass point away, and slide the rule to the right or left, at the same instant, following the division mark with the eye. The movement in each case should be three or four inches. If the rule has moved to the right and the compass point away from it, the imaginary line traced by the latter will seem a good deal curved and inclined to the left. If the rule has moved to the left, the line will appear nearly straight and inclined to the right. If the compass point approaches the rule instead of leaving it at the instant of movement, the results will be reversed, movement of the rule to the right giving inclinations to the right, and toward the left inclinations to the left. The movement must not be so fast as to prevent clear seeing of the relation of the rule and compass point. Invariable fixation of the division of the rule is important.

When this experiment is compared with *c* above, it will be observed that the inclination in this case is the same as that of the bands in Zöllner's figure when the pin is moved over them. The movement of the eye in following the pin head corresponds to that of the eye in following the rule. The heavy dark bands correspond to the imaginary line drawn by the compass point. The illusion of Zöllner's figure would, therefore, depend on the apparent motion of the bands, which in turn depends on the movement of the eyes. The dislocation of the lines in the case of the Zöllner's figure is, however, considerably less than in that of the rule and compasses, for such an excessive dislocation would make the lines appear to cross, a state of things that could not be harmonized with other parts of the perception.¹

Helmholtz, *A*, F, 727-730 (568-571).

¹Helmholtz thus seems to give two explanations for the same illusion. In explanation of this, he says (F, 730, (571)): "It may be found surprising that I should derive the same illusion from two causes so different in appearance. But if it is recalled that in my opinion the knowledge of the measurements of the visual field in indirect vision, rests upon experience previously had by the aid of movements, and that the present movements of regard are accompanied by similar new impressions, it is seen that the two cases are not so different as they may seem in exposition: they do not differ, except as the memory and present aspect of similar circumstances."

169. The geometrical illusions viewed with unmoved eyes. Many of the illusions considered above are much weakened, and some entirely removed when eye movements are excluded. This may be done by fixation of the eyes, somewhat better by getting the figures as after-images, and most satisfactorily of all by instantaneous illumination. Try the effect of steady fixation on the figures of Ex. 163-164.

The after-image method may be tried on the Zöllner's figure (first figure under Ex. 164), which can be made to give a strong after-image as it stands, and on any of the other figures by cutting them in narrow slits in black cardboard, and then viewing them against a bright background.

The fact that many of these illusions are still present in a certain degree when movements of the eyes are excluded, does not demonstrate that any part of them is of non-motor origin. Says Wundt (4te Aufl., II, 139): "If a phenomenon is perceived with the moved eye only, undoubtedly the influence of movement upon it is proved; but it cannot be inferred in the other way, as is now and then done, that movement is without influence on a phenomenon, that persists [with the eye] at rest." As has already been shown in Ex. 158, the experiences of the eye in motion are retained and applied to its perceptions when at rest.

Helmholtz, A, F, 725 ff. (567 ff.).

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APPENDIX.

THE FIELD OF REGARD AND LISTING'S LAW.

Experiment 158 of this section requires a somewhat fuller understanding of Listing's Law than can be gathered from Ex. 123, where the subject was previously treated. It has, therefore, seemed best to attempt a fuller exposition of it here.

Listing's Law as stated by Helmholtz, is as follows: "*When the line of sight passes from the primary position to any other position, the angle of torsion of the eye in its second position is the same as if the eye had come to this second position by turning about a fixed axis perpendicular both to the first and the second position of the line of sight.*"¹ On this principle rest two important corollaries: 1st. In movements from the primary position, there will be no rotation about the line of regard. 2nd. In movements from one secondary position to another, there will be some rotation about the line of regard.

The Hemispherical Field of Regard.

The usual way of putting the law to experimental test is to get a strong after-image of a rectangular cross on the centre of the eye, and then to observe the changes that its projected image undergoes as the eye is turned to one point and another of its field of regard. In the model from which the accompanying stereoscopic diagram is taken, an attempt has been made to show the changes

¹Helmholtz, *Optique physiologique*, p. 608, (466); Le Conte, *Sight*, 174.

that such an after-image would undergo when projected upon different parts of the hemispherical field. The primary meridian of this field is $A * B$,¹ and other meridians are shown at intervals of 20° . The equator of the field (that is the line of intersection of the plane of regard with the hemispherical field of regard, when the eyes are in the primary position) is $C * D$, and above and below it are shown parallels at intervals as before of 20° . The eye itself is supposed to be at the centre of the sphere, i. e., in the plane of the letters A, K, G, N , etc., and at the centre of the circle that they mark. When the eye is in its primary position, it is directed forward and fixed upon the central eight-rayed cross. Let us suppose that the eye takes a lasting after-image from the cross, but first from the horizontal and vertical rays only. If, now, the point of regard is elevated or depressed in the primary meridian, and there is no rotation about the line of regard, the vertical bar of the after-image cross will still be found to lie in the meridian; and if the point of regard be carried to the right or left in the equator of the field, the horizontal bar will still lie in the equator. This is shown by the slender crosses 40° from the centre on $A * B$ and $C * D$. The axes about which the eye turns are evidently in the plane of the letters A, K, G, N , etc., and coincide in the first case with the diameter CD , and in the second with the diameter AB . Suppose now, that the after-image has been taken from the oblique bars of the central cross, and that the movement of the eye has been oblique to the right and upward, and to the left and downward along $H * G$, and to the left and upward, and to the right and downward along $E * F$, but without rotation about the line of regard. As before, those bars of the cross which originally coincided with these lines will be found to coincide with them after the movement, as shown by the corresponding bars of the slender crosses in these positions. The axis for movements in $G * H$ lies in the diameter EF , and that for movements in $E * F$ in the diameter GH . For any intermediate directions of movement, the axes would have a corresponding intermediate position, but in all cases the axes would lie in the plane of the letters A, K, G, N , etc., perpendicular to the line of regard, both before and after its movement.

Since these after-images are always projected on a hemispherical surface there is no distortion of any of the crosses, and all of their parts maintain exactly the relations among themselves which exist among those of the central cross. It will be observed, however, that in the oblique positions the bars corresponding to the vertical of the central cross, do not quite coincide with the meridian passing through the centre of the crosses, but make a small angle with it, and that in the same way the bars corresponding to the horizontal in the central cross have no longer the same direction as the parallels above and below them. In other words the vertical and horizontal bars appear to have rotated, though the fact that the oblique bars have maintained their coincidence with the circles $E * F$, and $G * H$ shows that the rotation is not real, but as Le Conte says, "only an apparent rotation consequent upon reference to a new vertical meridian of space." This apparent rotation is known as *torsion*. The rules for this torsion are as follows: Movement of the eyes upward and to the right gives torsion to the right; upward and to the left, torsion to the left; downward and to the right, torsion to the left; downward and to the left, torsion to the

¹In naming the curves of the hemispherical field, the asterisk (*) is used for the central cross instead of a letter.

right—all of which can easily be observed in the stereoscopic figure.

So much for movements from the primary to a secondary position. Movements from any secondary position to the primary are evidently executed about the same axes as before, but in the contrary direction. It remains then to consider movements from one secondary position to another. Let us start with an after-image from the slender cross on $C \cdot D$, 40° to the right of the centre, and move upward along the meridian. The vertical bar of this cross coincides with the meridian at the start; when we reach the position of the eight-rayed cross, however, it no longer does so, but has turned slightly to the right—this time owing to a true rotation about the line of regard, and not to reference to a new meridian. The amount of rotation is small, in this case about 13° . Movement downward along the meridian would have exactly the same result, except that the rotation would be in the opposite direction, and similar rotations would be found if the cross 40° to left of the centre on $C \cdot D$ had been used for vertical movements, or the crosses 40° above and below the centre on $A \cdot B$ had been used for horizontal movement along great circles.

If movements from secondary positions along great circles are attended with this deviation of the bar of the cross from the line in which it moves, are there any lines to be found along which the eye may move the after-image without finding such a deviation? There are such lines, and four of them are shown in the figure. They are the arcs IJ , KL , MN and OI . It will be seen that these are drawn through the sloping positions of the bars corresponding to the vertical and horizontal bars of the central cross, and are perpendicular to $A \cdot B$ and $C \cdot D$ like the bars of the side crosses. Along these lines, a short line or after-image can be moved without leaving the line, a peculiarity in which they resemble a straight line, and when seen with the eye at rest under proper conditions they do actually appear straight. These are the Circles of Direction or Right Circles, (*Cercles de Direction*, *Richtkreise*) of Helmholtz.¹ The verticle circles of direction have, it will be observed, somewhat greater curvature than the meridians through the same points, and the horizontal circles of direction somewhat less than the parallels near which they lie. These circles have the further peculiarity that they all pass through the occipital point, a point as far behind the eye as the primary point of regard is in front of it. Both of these properties are shared also by all the great circles passing through the primary point of regard, so that they also are circles of direction. Circles of this kind, great or small as the case may be, can be passed through any two points in the field, and are not limited to those shown in the figure.

The mathematical study of Listing's Law shows that the movement from one secondary position to another may, like those from the primary position, be conceived as rotations about fixed axes all of which lie in a plane, (though in this case the plane is not perpendicular to the line of regard), and that in every case there is also a line about which there is no rotation, the *atropic line*, though this does not coincide with the line of regard.

The Plane Field of Regard.

The experimental testing of Listing's Law is generally carried out with the plane, instead of the hemispherical, field of regard,

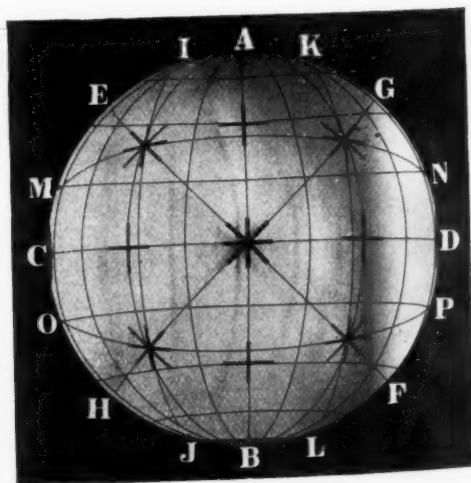
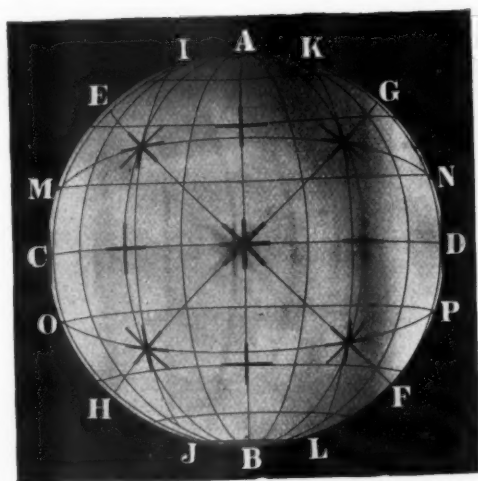
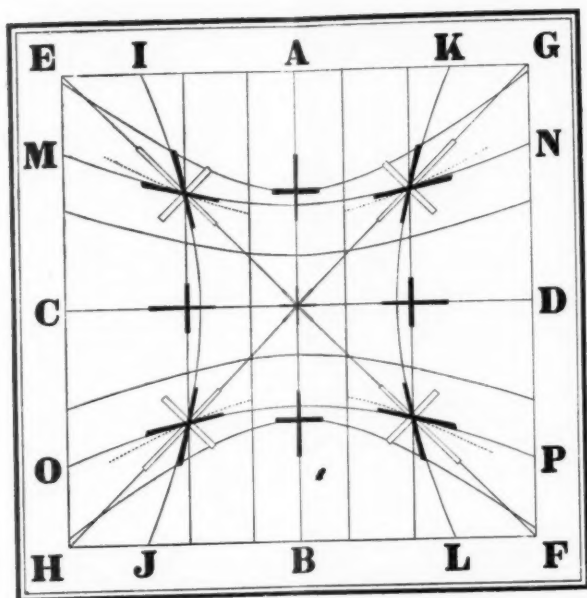
¹*Op. cit.*, 636-637, 703-713, (492-493, 548-557.)

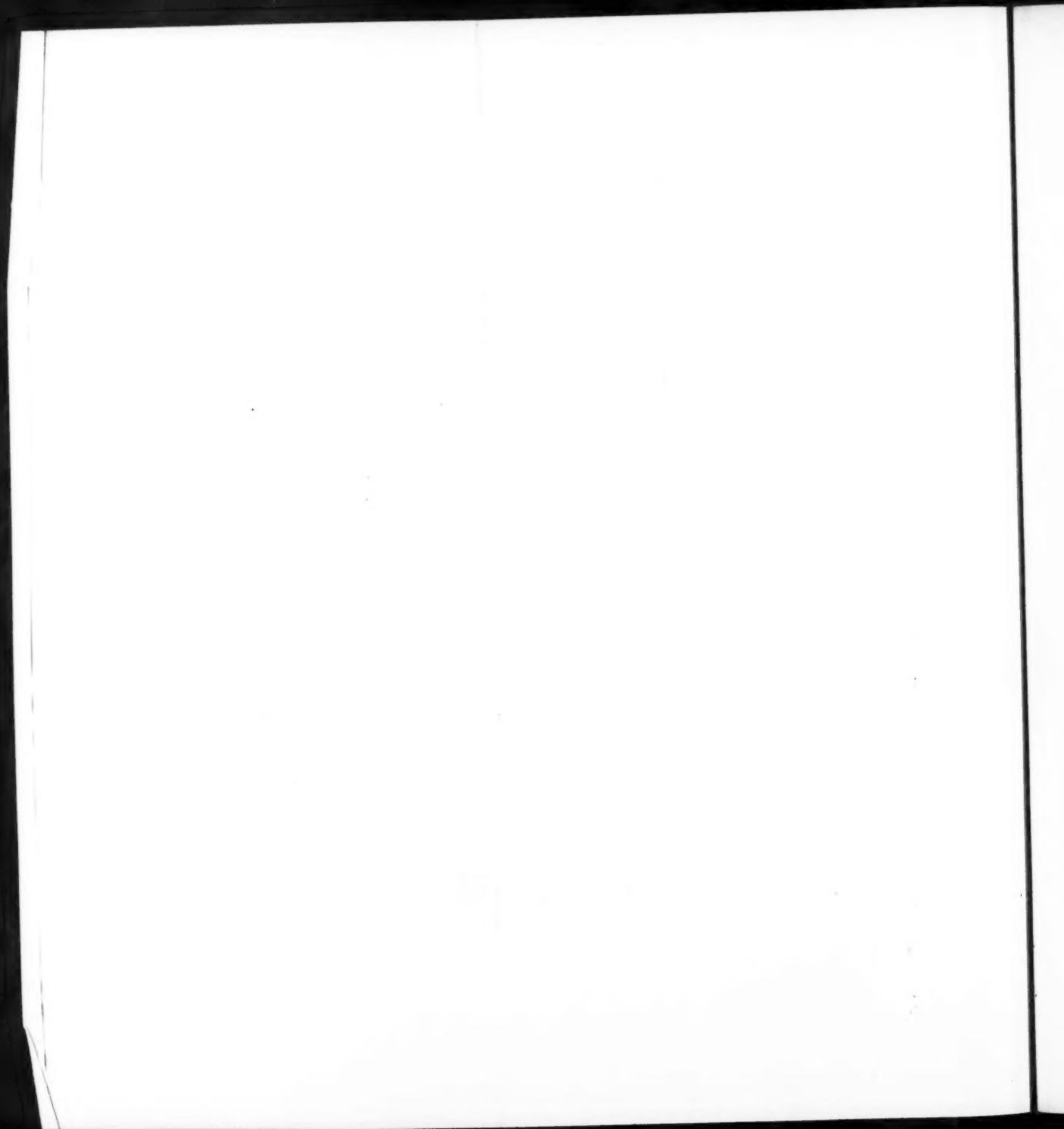
because of the difficulty of providing a large enough hollow hemisphere. But this has the disadvantage of adding to the changes in the after-image due to the movements of the eyes, a wholly new set of distortions due to the projection of the image upon an oblique surface. These are easily seen in the figure for the plane field.

This figure is a gnomonic projection of the hemispherical field upon a plane tangent to it at the middle point of the central cross. On this plane all the lines of the hemispherical field are represented exactly as their shadows would be cast by a point of light in the place of the eye, *i. e.*, in the centre of the sphere. The meridians are represented by vertical straight lines, wider and wider apart as they are removed from the primary meridian *AB*. The parallels become hyperbolas, increasing in curvature as they are more distant from the equator of the field. The great circles through the primary point of regard are straight lines through the same point. The other circles of direction are hyperbolas. They maintain their resemblance to straight lines, however, in so far as concerns a short linear after-image moved along them, and are called by Helmholtz the *right-lines* of the field of regard. The lettering of all the lines in the two figures is the same, so that comparison will be easy.

The distortion of the crosses on *AB* and *CD* is easy to understand, and also the oblique bars of those on *EF* and *GH*. The bars corresponding to the vertical and horizontal arms of the central cross—in solid black in the figure—require a little explanation. If the matter were one of simple projection, without torsion, the bar corresponding to the vertical ought to coincide with the projection of the meridian, and that corresponding to the horizontal bar ought to coincide with the projection of the line cutting the meridian at right angles in the hemispherical field, *i. e.*, the projection of the parallel that passes through the centre of the cross—the dotted lines in the figure. When these things are regarded it is found that both arms of the cross show torsion as in the hemispherical field, though the distortion due to projection seems at first to have turned the two arms in opposite directions.

This exposition has necessarily been physiological and geometrical. The psychological interest in the matter depends on the fact that the perception of space with the eye at rest is profoundly affected by its experiences in motion, a large group of which are received while the eye is functioning in more or less accord with Listing's Law. For a fuller account of these psychological matters, see Ex. 158.





PROCEEDINGS OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

THIRD ANNUAL MEETING HELD AT PRINCETON UNIVERSITY,
PRINCETON, N. J., DECEMBER, 27 AND 28, 1894.

Report of the Secretary and Treasurer for 1894.

The third annual meeting of the American Psychological Association was held at Princeton College, Princeton, N. J., on Dec. 27 and 28, 1894. Prof. William James, president of the association, presided over the sessions, which lasted from 10.30 A. M. on Dec. 27 to 4.30 P. M. on Dec. 28. President Patton of Princeton College made an address of welcome on Thursday afternoon, and entertained the members of the association in the evening after the address of the president of the association. Abstracts of the papers read at the meeting are subjoined. Papers by Prof. Starr and Prof. Hume were presented in the absence of their authors and papers offered by Prof. Jastrow, Prof. Delabarre, Prof. Titchener, Mr. Pierce and Dr. Witmer were not read.

The members in attendance were Alexander, Baldwin, Cattell, Chrysostom, Farrand, Hyslop, Franklin, James, Ladd, MacDonald, Marshall, Mead, Mezes, Mills, Miller, Newbold, Ormond, Pace, Royce, Sanford, Strong, Warren—twenty-two in all.

The following nominations for membership were made by the council and the elections were made by the association :

Prof. Archibald Alexander, New York ; Dr. John Bigham, University of Michigan ; Prof. Charles L. Dana, Bellevue Medical College ; Mr. E. A. Kirkpatrick, Winona, Minn. ; Dr. A. Kirschmann, University of Toronto ; Prof. S. E. Mezes, University of Texas ; Mr. W. Shaw, Wesleyan University ; Prof. James Seth, Brown University ; Prof. Paul Shorey, University of Chicago ; Prof. H. M. Stanley, Lake Forest University ; Dr. Margaret Washburn, Wells College.

A constitution was adopted as follows :

CONSTITUTION OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

ARTICLE I. *Object.* The object of the association is the advancement of psychology as a science. Those are eligible for membership who are engaged in this work.

ART. II. *The Council.* A council shall be elected from the members of the association as an executive. The council shall consist of six members, two being elected annually for a term of three years. The president shall be *ex-officio* a member of the council. The council shall nominate officers for the association, shall nominate new members and shall make other recommendations concerning the conduct of the association. The resolutions of the

council shall be brought before the association and decided by a majority vote.

ART. III. *Officers.* There shall be annually nominated by the council and elected by the association a president, and a secretary, and a treasurer, who shall perform the usual duties of these officers.

ART. IV. *Annual Subscription.* The annual subscription shall be three dollars (\$3.00) in advance. Non-payment of dues for two consecutive years shall be considered as equivalent to resignation from the association.

ART. V. *Executive Committee.* The president, the secretary and a member from the place where the meeting is held shall be a committee to make necessary arrangements for the annual meeting.

ART. VI. *Proceedings.* Such proceedings shall be printed by the secretary as the association may direct.

ART. VII. *Amendments.* Amendments to the constitution must be adopted by a majority vote at two consecutive annual meetings.

As prescribed by the constitution, a council was elected as follows:

Term expiring 1897:

Prof. G. T. Ladd, Yale University.

Prof. J. McKeen Cattell, Columbia College.

Term expiring 1896:

Prof. J. Mark Baldwin, Princeton University.

Prof. William James, Harvard University.

Term expiring 1895:

Prof. John Dewey, University of Chicago.

Prof. G. S. Fullerton, University of Pennsylvania.

Prof. J. McKeen Cattell was elected president, and Prof. E. C. Sanford, secretary and treasurer for the coming year.

An invitation was received from the American Society of Naturalists inviting the association to affiliate with it. The question was referred to the council with power to act. Invitations were received for the meeting of 1895 from Harvard University and from the University of Chicago. The decision as to place of meeting was left with the council, with the recommendation that the association meet, if possible, at the same time and place as the Society of Naturalists. It was resolved that the minutes should be printed in such journals as were prepared to print them in full.

The report of the treasurer is as follows:

RECEIPTS.	
Balance on hand,	\$69 50
2 dues 1893,	6 00
38 dues 1894,	114 00
Sales of proceedings,	1 60
	<hr/> \$191 10

EXPENDITURES.	
Printing Proceedings for 1893, as per	
Messrs. Macmillan & Co.'s voucher,	\$55 93
Postage, expressage and stationery,	8 00
	<hr/> 63 93

Balance on hand,	\$127 17
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The account was audited by the council and approved.

J. McKEEN CATTELL,
Secretary, 1894.

ABSTRACTS OF PAPERS.

- (1) *The Knowing of Things Together*. Address by the President, PROF. WILLIAM JAMES, Harvard University.

The synthetic unity of consciousness is one of the great dividing questions in the philosophy of mind. We know things singly through as many distinct mental states. But on another occasion we may know the same things together through one state. The problem is as to the relation of the previous many states to the later one state. It will not do to make the mere statement of this problem incidentally involve a particular solution as we should if we formulated the fact to be explained as *the combination of many states of mind into one*. The fact presents itself in the first instance as *the knowing of many things together*, and it is in those terms that the solution must be approached.

In the first place *what is knowing?*

1. *Conceptual* knowing is an external relation between a state of mind and remote objects. If the state of mind, through a context of associates which the world supplies, leads to the objects smoothly and terminates there, we say it knows them. 2. *Intuitive* knowing is the identity of what taken in one world-context we call mental content and in another object. In neither 1 nor 2 is there involved any mysterious self-transcendency or presence in absence. 3. This mystery does, however, seem involved in *the relation between the parts of a mental content itself*. In the minimum real state of consciousness, that of the *passing moment*, past and present are known at once. In desire, memory, etc., earlier and later elements are directly felt to *call for* or *fulfill* each other, and without this sense of mutuality in their parts, such states do not exist. Here is presence in absence; here knowing together; here the original prototype of what we mean by knowledge. This ultimate synthetic nature of the smallest real phenomenon of consciousness can neither be explained nor circumvented.

We can only trace the particular conditions by which particular contents come thus to figure with all their parts at once in consciousness. Several attempts were then briefly passed in review. Mere synchronical sense-impression is not a sufficient condition. An additional inner *event* is required. The event has been described: *physiologically* as (1) attention; as (2) ideational processes added to the sensorial processes, the latter giving unity, the former manyness; as (3) motor synergy of processes; *psychologically* as (4) the thinking of relations between the parts of the content-object; as (5) the relating of each part to the self; *spiritually* as (6) an act of the soul; *transcendentally* as (7) the diminution (by unknown causes, possibly physiological) of the obstruction or limitation which the organism imposes on the natural knowing-of-all-things-together by an Absolute Mind. For transcendentalism the problem is "how are things known separately at all?"

The speaker dealt with these opinions critically, not espousing either one himself. He concluded by abandoning the attempt made in his *Principles of Psychology* to formulate mental states as integers, and to refer all plurality to the objects known by them. Practically the metaphysical view cannot be excluded from psychology-books. 'Contents' have parts, because in intuitive knowledge contents and objects are identical; and psychology even as a 'natural science' will find it easier to solve her problem of tracing the conditions that determine what objects shall be known together by speaking of 'contents' as complex unities. [The address will be printed in full in the *Psychological Review* for March, 1895.]

- (2) *Minor Studies and Notes on New Apparatus.* By DR. E. C. SANFORD, Clark University.

The four papers reported were on the following topics: 1. Comparative Observations on the Indirect Color Range of Children, Adults, and Adults Trained in Color; by Geo. W. A. Luckey. (This study was made in the psychological laboratory of the Leland Stanford, Jr., University.) 2. A Study of Individual Psychology, by Miss Caroline Miles. 3. The Memory Span and Attention, by Dr. Arthur H. Daniels. 4. On the Least Observable Interval Between Stimuli Addressed to Disparate Senses and to Different Organs of the Same Sense, by Miss Alice J. Hamlin; 5 Notes on the Binocular Stroboscope, a Model of the Hemispherical Field of Regard, and Diagrams for an Optical Illusion by E. C. Sanford. [All of these papers are printed in full in this number of the JOURNAL.]

- (3) *The Psychic Development of Young Animals and its Physical Correlation.* By T. WESLEY MILLS, Professor of Physiology in McGill University, Montreal.

As the comparative method of embryology and the doctrine of organic evolution have revolutionized biology, it must be expected that they or their analogues will at least greatly modify modern psychology. To learn how and when psychic processes originate is a long step towards understanding them; and as these processes in animals lower in the scale than man are presumably simpler, it is desirable that they be studied both in the mature animal and in the young developing one. Accordingly the writer has for some years been engaged in this task and has now made fairly complete researches on the psychic development of the dog, cat, rabbit, guinea-pig, etc. An attempt has been made to keep a record, in the form of a diary, not only of psychic but of contemporaneous physical changes. A special series of experiments has been made on the brains of young animals with a view of determining when cortical localization is established, in what order, etc. This work is not yet complete. Incidentally the subject of localization in the mature animal has been investigated and some generally accepted conclusions found unreliable, as well as others confirmed.

- (4) *On the Distribution of Exceptional Ability.* By Professor J. McKEEN CATTELL, Columbia College.

A study of the mental traits, and of the works of great men forms an interesting chapter in psychology, and while we are undertaking to make psychology an exact science, it is an advantage to secure quantitative results. When anecdotes are published telling us that certain great men have inherited or bequeathed their talents, were insane, immoral, precocious, versatile, or the like, it is of interest, but we sometimes imagine that other examples might be quoted with opposite results or similar traits found in ordinary people. We need to be able to affirm that a man, who has accomplished work making him eminent, is more likely to be insane (according to a proper definition of insanity), than the average man, in a given ratio, and that this ratio varies in such and such a way for men whose work or character was of a given definable sort. And so in all cases quantitative results should be secured. We should be able to say that a man who is a great painter is just so much more likely to be a great poet as well, than is a great soldier or than is the average man.

The first requirement for such a study is a list of great men

secured by an objective method. The 1000 most eminent men have been selected by collating the space given to them in different biographical dictionaries and encyclopædias. The method secures impartiality and an assignable degree of accuracy, it being possible to give a probable error to each man. The list, of course, only gives a man's place in contemporary interest, but this would agree closely with the average verdict of the best judges as to his importance in history. The exact composition of the list is not, indeed, a matter of much importance for the end in view—an objectively selected list of great men being what is wanted. The list was shown at the meeting, curves were exhibited demonstrating the distribution in time and race of the 1000 men, and attention was called to some facts brought out by the curves.

- (5) *Sensibility to Pain by Pressure in the Hands of Individuals of Different Classes, Sexes and Nationalities.* By Dr. ARTHUR MACDONALD, Bureau of Education, Washington.

TABULAR STATEMENT OF RESULTS.

No.		Total No.	RIGHT HAND.			LEFT HAND.		
			No. Requiring More Pressure in l. h.	Totals in Kilogrammes.	Averages, Kilogrammes	No. Requiring More Pressure in l. h.	Totals, Kilogrammes.	Averages, Kilogrammes.
1	2	3	4	5	6	7	8	9
1	American Professional Men,	20	14	74.50	3.72	5	65.25	3.26
2	American Business Men,	14	6	85.25	6.08	6	87.75	6.05
3	American Women—non-laboring class,	27	13	93.25	3.45	6	91.83	3.38
4	English Professional Men,	17	9	88.50	5.20	6	87.25	5.13
5	English Women—non-laboring class,	7	4	43.00	6.14	2	44.25	6.32
6	German Professional Men,	6	5	31.25	5.20	1	29.00	4.83
7	Salvation Army members, London,	8	6	73.25	9.15	2	51.00	7.62
8	Slum Men in Chapel-Rouge, Paris,	9	3	122.50	13.61	2	119.50	13.27
9	Boston Army of the Unemployed,	34	16	332.50	9.77	14	333.75	9.81
10	Women in "Maisons de Tolance," Paris,	9	3	82.00	9.00	5	84.25	9.36
11	Epileptic Patients—laboring people,	3	1	28.00	9.33	1	27.00	9.00
12	Odd ones, men in Paris,	7	4	28.25	4.03	3	26.25	3.75
13	Odd ones, men in different countries,	18	10	96.25	5.34	5	89.50	4.97
14	Men in general,	142	76	1012.75	7.13	19	979.50	6.89
15	Women in general,	46	21	230.50	5.01	15	233.08	5.06

The preceding experiments were made incidentally upon different classes of people. Quite a number of university specialists interested in the subject were experimented upon. The middle of the palmar fossa was chosen and Prof. Cattell's algometer was employed.

Should these results be proved to be generally true by experiments on larger numbers of people, the following statements would be probable:

I. The majority of people are more sensitive to pain in their left hand. (Only exception is No. 10, cols. 4 and 7.)

II. Women are more sensitive to pain than men. (Nos. 14 and 15, cols. 6 and 9.) Exceptions are: comp. Nos. 4 and 5, cols. 6 and 9. It does not necessarily follow that women can not endure more pain than men.

III. American professional men are more sensitive to pain than American business men (comp. Nos. 1 and 2, cols. 6 and 9); and also than English or German professional men (comp. Nos. 1, 4 and 6, cols. 6 and 9).

IV. The laboring classes are much less sensitive to pain than the non-laboring classes (comp. Nos. 1, 2 and 9, cols. 6 and 9.)

V. The women of the lower classes are much less sensitive to pain than those of the better classes (comp. Nos. 3, 5 and 10, cols. 6 and 9).

VI. In general, the more developed the nervous system, the more sensitive it is to pain.

Remark: While the thickness of the tissue on the hand has some influence, it has by no means so much as one might suppose *a priori*; for many with thin hands require much pressure (Nos. 5 and 10, cols. 6 and 9).

(6) *The Freedom of the Will.* By BROTHER CHRYSOSTOM, Manhattan College, New York.

The positive results of the latest studies of the will, through introspection and experiment, are in striking accord with the teachings of the Schoolmen. The appetencies of Aristotle have been replaced by conation, which, if considered in the form of attention, is either *univocally conditioned*, and then corresponds to the sensitive appetition of scholastic philosophy, or is *equivocally conditioned*, and then does not essentially differ from the volition of earlier philosophers. But since equivocally conditioned attention may include among the objects attended to even the attending subject, it must be a spiritual action, for matter is incapable of such reflexive process. In other words, the attending mind is a rational soul. In this light, *apperception* may be characterized as the distinctive quality of conation. But apperception supposes at least such intellectual action as is contained in conception, and this in turn supposes sensation; and thus a point of contact is made with Münsterberg's theory.

Neither a purely autogenetic nor a purely heterogenetic theory of will accounts for all the facts. For conation is not a mere combination of sensations, nor a resultant of affection and sensation, nor does it consist in affection alone. Again, peripheral excitation fails to account for the active element of conation, while exclusively central excitation overlooks external influence. We must, then adopt a theory midway between these two extremes. Wundt, therefore, must be held to state rather the physiological correlate than the psychical fact.

The chief difficulty as to the freedom of the will is found in its connection with the law of causality, which law, however, belongs

to the domain of metaphysics, only indeterminism coming within the limits of psychology. *Cause* essentially connotes the inflowing of the agent upon some subject. But *free* and *uncaused* are not synonyms. All action of the will is voluntary, yet not all its action is free. For although the presentation of pleasurable or painful objects to the will, *i. e.*, the motives, together with the agent's temperament and general subjective condition determine the spontaneous impulse of his will, yet it is a fact of conscious experience that he often can and does put forth at the same time an anti-impulsive effort. Only actions made under these conditions are rightly called free, and they imply essentially the power to will or not to will.

Yet the law of causality, even in that narrower meaning, which obtains in the physical sciences, also applies to free actions in the mass, for we can determine with more or less probability what men taken generally will do under given circumstances. In conclusion, Wundt's assertion that a free act is necessarily an uncaused one, is virtually an admission that the will is superior to material force, and is therefore spiritual.

(7) *The Consciousness of Identity and So-Called Double Consciousness.* By Prof. GEORGE T. LADD, Yale University.

The questions in debate concerning the consciousness of identity and so-called double consciousness can not be intelligently discussed without a critical examination of the conceptions involved. What then do we mean when we speak of a thing or a mind as remaining "identical" or self-same, through various changes of states? To uncritical thought it doubtless seems as though some unchanging "core" of reality belonged to every being of which we feel ourselves entitled to speak in this way. But philosophical criticism seems rather to assure us only of the proposition: *The real identity of anything consists in this, that its self-activity manifests itself in all its different relations to other things as conforming to law, or to some immanent idea.*

From this it follows that change, in itself, is not inconsistent with identity being maintained. On the contrary, it is the very character of the actual changes observed or inferred, which leads either to the affirmation or to the denial of identity. This principle may be applied to whatever is popularly called a thing, and also to those hypothetical elements of all material things, the so-called atoms.

When we turn to consider the peculiar identity of mind, we find that the affirmation of such identity can never be taken as a denial of change. Indeed, the very real being of mind seems dependent upon change,—in the form, namely, of successive states of consciousness. So that the variety and greatness of the changes experienced may heighten, rather than diminish the reality and validity of the consciousness of identity, properly described and understood.

Now, if we inquire in what consists this conscious identity, we see that it is, and can be, nothing but that which is given to consciousness in all states of self-consciousness, of recognitive memory, and of reflective thinking about the Self. To have these states of consciousness is to be conscious of being identical and self-same. And degrees of the consciousness of identity, as it were, are connected necessarily with all real mental development.

In accordance with this metaphysical analysis, we may hopefully and even confidently venture upon the attempt to account for the phenomena of so-called double consciousness, in accordance with certain well-known psychological principles. Of these one may be spoken of as the principle of "psychic automatism." Under this

principle, we note in many of our most familiar experiences such a diremption of successive states, or of very complex present states into two-fold combinations of elements, as makes the full impression of *two* interacting personalities rather than of one person. Yet very subtle and unrecognized or dimly recognized influences of one upon the other, of the Self-conscious Ego upon the automaton, or the reverse, may be distinguished by psychology. All this is popularly expressed either by saying, "I have the automaton," or "the automaton has me;" "I am the automaton," or "the automaton is not-me." Illustrations of all this may be derived from the simpler or more complex bodily operations as under the influence of semi-conscious states, and in turn influencing them; from many deeds of skill and valor and even of a seemingly high order of intelligence; from the phenomena of artistic and religious inspiration, etc.

Closely akin to this is the most effective working of another principle, which we will call that of a "dramatic sundering of the Ego." We can more or less consciously and intentionally, or as forced by circumstances, so "put ourselves into" another character as virtually to divide the Self into two or more selves, whose appropriate states of consciousness either follow in rapid succession or seem to occur almost simultaneously. The phenomena of dreams, the plays of children, the experience of many actors, the phenomena of certain states of inspiration, the imaginative genius of certain writers, like Balzac notably, are instances in point here. Indeed, the very nature of ethical consciousness in its highest form of manifestation seems necessarily to involve such a dramatic sundering of the Ego. In not very infrequent cases, three interacting personalities become manifest in consciousness. These may be described as the tempter or bad angel, the good angel, and the self as the "torn one" between the two.

In fine, it seems fair to expect that by a further understanding and more extended application of these, and perhaps other cognate psychological principles, even the most extreme hypnotic cases of so-called double consciousness may finally be explained.

- (8) *A Preliminary Report on a Research into the Psychology of Imitation.* By Prof. JOSIAH ROYCE, Harvard University.

This report first briefly described a collection of experiments now under way at the Harvard Psychological Laboratory, and then passed to some reflections suggested by these experiments, relating to the definition of the functions to be grouped together under the name of imitation. As the text of the report is to appear in the *Psychological Review* the present summary need not be extended. The experiments, which at present are only in their first beginning, have thus far been confined to the imitation of somewhat complex series of taps, given by an electric hammer, and arranged in rhythms. The subjects of the experiments imitate the taps, after hearing each rhythm, through repeating the hammer-strokes by means of an electric key. The rhythms as given and as imitated are recorded on the kymograph. The effects of habit in successive imitations of the same rhythm, the influence of speed and of other factors upon success in imitation are under study. The complexity of the rhythms studied in these experiments forms one special difference of this enterprise when compared with other experimental studies of rhythm; for the purpose is to study, not the rhythmic consciousness as such, but the imitative functions.

Notes of subjective experiences, taken down during or imme-

diately after each experiment, by the subjects concerned, have already given the suggestion for those considerations concerning the definition of imitation, with which the major part of the report was taken up.

- (9) *The Classification of Pain.* By Prof. CHARLES A. STRONG, University of Chicago.

This paper was a discussion of the current theory that pleasure and pain are always given as aspects of a content distinct from themselves—the feeling-tone, “*quale*,” or aspect theory. It sought to test this theory by considering its application to the case of cutaneous pain.

1. Neurologically, we know no facts in regard to cutaneous pain which decisively contradict the theory. For special pain-nerves are more than doubtful; and there is a symptom of locomotor ataxia, consisting of hyperalgesia to heat or cold without hyperalgesia to pressure, and even with analgesia to pricking and pinching, which seems to prove that some pains are distinctively pains of temperature. The condition of analgesia, moreover, while it implies distinct paths for pain in the spinal cord, may be reconciled with the aspect theory by holding that the sensation called forth through these paths, is a tactile or temperature sensation in painful phase.

2. But, introspectively, it is impossible in certain cases to carry out the analysis for which the aspect theory calls. Extreme pressure, heat and cold produce the same sensation—not of heat, or cold, or pressure, but simply of pain. This sensation (*Schmerz*) does not admit of analysis; it is impossible to separate it into a content and an accompanying feeling-tone. But it may call forth an emotional reaction in the shape of a feeling of the disagreeable or intolerable (*Unlust*).

In conclusion, the inference was drawn that pain, being a sensation, may be localized and may leave behind images. [The paper will be printed in the *Psychological Review* for May, 1895.]

- (10) *A Theory of Emotions from the Physiological Standpoint.* By Prof. G. H. MEAD, University of Chicago.

Professor Dewey having shown that it is possible to make a complete teleological statement of the emotions along the line of the discharge theory, it is interesting to see how far such a statement may be paralleled by a physiological theory. This would involve also a physiological theory of pleasure and pain. As pain can be differentiated from the sensations in connection with which it generally appears in consciousness, as it shows itself under circumstances in which the tissue of the end organs or the nerves themselves are affected, and as in the diseases, in which we find pain as a constant concomitant those parts are affected, which are richly supplied with blood vessels by means of supporting and nourishing tissues (*Rindfleisch's intermediärer Ernährungsapparat*), and as in those diseases which pass usually without pain (as in the catarrhs of the various mucous membranes) the tissues affected are poorly supplied with such blood-vessels, and enter into relation with the capillaries generally through the lymph, for the purpose of secretion, it becomes at least probable that, physiologically, pain may be considered as the interference through poisons or violence or otherwise with the process of nutrition as carried out in the finer arteries and blood-vessels. Pleasure must from this standpoint be considered as physiologically the normal or rather heightened process of nutrition in the organs, and the nerve paths

which connect these with the central nervous system, would be probably the sympathetic.

In the simple instinctive act that lies behind every emotion, the vaso-motor system is called into action by the enlargement of the small blood-vessels in the muscles and sweat glands. To maintain the blood pressure, the finer blood-vessels in the abdominal tracts are closed by the constrictors of that region, and the action of the heart may also be increased by the accelerators. The vaso-motor system thus is, in these simpler instinctive acts, in automatic connection with the senso-motor. The act must commence before the flow of blood can take place. It is in connection with this increased flow of blood, that we have to assume the emotional tones of consciousness arise according to the discharge theory. Within the act it would answer only to interest. It is in the preparation for action that we find the qualitatively different emotional tones, and here we find increased flow of blood before the act. We find also what we may term symbolic stimuli, which tend to arouse the vaso-motor processes, that are originally called out only by the instinctive acts. These stimuli in the form in which we can study them, seem to be more or less rhythmical repetitions of those moments in the act itself, which call forth especially the vaso-motor response. In this form they are recognized as æsthetic stimuli, and may be best studied in the war and love dances. It is under the influence of stimuli of this general character that the emotional states and their physiological parallels arise. The teleology of these states is that of giving the organism an evaluation of the act before the co-ordination that leads to the particular reaction has been completed.

- (11) *Desire as the Essence of Pleasure and Pain.* By Dr. D. S. MILLER, Bryn Mawr College.

Pleasure and pain, in the discussion now going forward, as to their classification and physical basis, are commonly treated as among our passive sensory experiences; at all events, it would seem to most psychologists a somewhat stupid paradox to assert that they were in any sense motor phenomena. Yet there is solid ground for holding this paradox; for maintaining, at least, that pleasantness (the quality which along with their specific differences of character marks all so-called pleasures) and painfulness (the quality which along with their specific differences of character marks all so-called pains) are essentially motor facts. A pain is an intolerable feeling; different as they are among themselves, all pains have this, at least, in common, that they are intolerable. No other feeling is intolerable; if it were we should call it a pain. It would then, not be easy to refute the proposition that painfulness is intolerableness; that so-called pains have no other common class-attribute. Now intolerableness is the quality of uniformly provoking a certain bodily disquietude or rebellion, issuing, where the nature of the case permits, in an attempt to escape from the offending irritant. And this is a motor phenomenon. The various *disagreeables* (a term with which "pains" in my meaning is convertible) a needle-prick, a headache, a burn, the numb internal ache of cold hands, the taste of quinine, the smell of assafœtida, the scratching of a slate-pencil, "gnawing pains," "shooting pains," muscular fatigue, disappointment, humiliation—these have no such intrinsic resemblance in sensational complexion as we find among different sights or sounds—between the members of the class of visual, or of the class of auditory sensations; they are similar only in the extrinsic fact that they all alike are accompanied by a bodily

reaction—some flinching or shuddering or convulsion, some restiveness or inner tension—which tends then and afterwards to pass into movements of avoidance, escape or repulse. Now, these movements and the tendencies to them are what we know as *aversion* in its various forms and degrees.

If painfulness is intolerableness, pleasantness on similar grounds, is the quality of being *welcome*. The bodily reaction of *gusto* is as characteristic, though not so obtrusive as that of intolerance; and it tends to pass into movements of retention or procurement. These movements and the tendencies to them are what we know as *desire* in its various forms and degrees.

(12) *Pleasure and Pain Defined.* By Prof. SIDNEY E. MEZES, University of Texas.

It is necessary to find some fact or group of facts that is present whenever we experience pleasure and absent whenever we do not, and another fact or group of facts present and absent with pain. The frequent confusion of unpleasants with pains is very misleading. Unpleasants are of three kinds: memories and expectations; sensational unpleasants that are not pains—bitter tastes, e. g.; and sensational unpleasants that are pains—a toothache, e. g. We have here to define pleasure and the unpleasant. Attempts have been made to define pleasure-pains as sensations, as emotions, and as making up the genus of which sensations and emotions are two species. The fact that there is evidence for each of the first two theories shows that neither is exhaustive and competent. Besides the existence of pleasant and unpleasant memories, expectations, and fancies, invalidates all three. Many hold that pleasure-pains are ultimate ideas, simple and undefinable, like colors. There are strong positive objections to this theory, but negatively and for our purposes, it suffices that this theory is a last resort and that its supporters must overthrow all other theories before legitimately claiming it as established. This theory is valuable and true in so far as it points out that neither pleasures as a whole, nor unpleasants as a whole, have any properties in common. It overlooks the possibility that there may be something invariably co-present with pleasures, and some other invariably co-present with pains, and that these two may be the signs to us of the presence of pleasures and pains—what induces us to call a state pleasant or unpleasant. Now Plato, Aristotle, Hobbes, Kant and Schopenhauer agree that harmony or good adjustment is the mark of pleasure, ill-adjustment that of pain. Not all these writers point out the terms between which the adjustment is to obtain, but recently Wundt and Ward have held that the adjustment is of attention to its object. This immediately plausible suggestion of attention and adjustment must be examined. Clearly, what is not attended to is indifferent since uninteresting. Further, immediate attention to pleasures is not the same as that to pains; the former is easy and natural, the latter enforced and obstructed. Again, derived attention, always to unpleasants, is invariably obstructed by the more pleasant rivals to attention also present. May it not be that attention without obstruction is the mark of pleasure, attention with obstruction that of pain? The evidence for this view may be thus suggested: All states of intensely concentrated attention are pleasant, hard thinking, hard play, strenuous work; all states of *internal conflict*—hesitation, practical puzzle, co-present irreconcilable impulses, morbidly insistent ideas, etc.,—are unpleasant, and further, physical pains, owing to their great intensity, reverberate widely and naturally set up mutually obstructive reflexes.

- (13) *Emotions versus Pleasure-Pain*. By Mr. HENRY RUTGERS MARSHALL, New York.

Mr. Marshall reviewed his "genetic" argument in relation to the Emotions, emphasizing the contention that the typical Emotions are named, because (1st) they correspond to relatively fixed relations between the physical elements reacting, and because (2d) these reactions are immediate. Failure of these two conditions can be traced where "instinct feelings" have no emotional names. Emotions are in their nature irregular in recurrence, and to be of value must be forceful in reaction; hence Emotions are not usually lost to consciousness as many "instinct feelings" are, although, if these Emotions become rhythmical and weak, they act as other states do in relation to fixity of habit. Pleasure and pain relate to *organic*, while Emotions relate to *individual* or *racial* effectiveness or ineffectiveness; therefore their genesis cannot be considered to have been co-incident in time, nor to be of the same type.

The identification of Emotion and Pleasure-Pain in "Feeling" is dependent upon the validity of the tripartite division of mind; which is upheld by metaphysical postulation, but not by psychological evidence. Prof. Croom Robertson argued that the exhaustive categories, The True, The Good, The Beautiful, themselves proved the validity of the division. But the existence of the division is explicable in quite another way, as due to the search for Reality. In relation to mental experience in general this search gives us The True; in relation to impression it gives us The Beautiful; and in relation to expression it gives us The Good. If we are to discard this classical tripartite division, we should be able to account for its persistence. It results from an attempt to unify two diverse classifications, both bipartite; viz., 1st, the receptive-reactive classification, and 2d, the subjective-objective classification:—Sensation and Intellect (Knowing) being bound together on both the receptive-reactive and on the subjective-objective schemes; Pleasure-Pain and Emotion (Feeling) being bound together on the subjective-objective scheme, the receptive-reactive quality being unmarked; Will being marked by a common and co-ordinate emphasis of the reactive and also of the objective qualities. The existence of this tripartite division, thus explained, can therefore no longer be used as an argument for the bond between Emotion and Pleasure-Pain, which states are distinctly separable, the relation between them being this: The emotions are complex psychoses which almost invariably involve repressions or hypernormal activities, either of which are determinants either of pleasure or of pain.

- (14) *Notes on the Experimental Production of Hallucinations and Illusions*. By Prof. W. ROMAINE NEWBOLD, University of Pennsylvania.

Prof. Newbold reported that in twenty-two out of eighty-six cases tried he had succeeded in producing illusions by causing the patients to gaze into a transparent or reflecting medium, such as water, glass, and mirrors. His most successful cases were found among young women under twenty years of age who were good visualizers, but as a majority of his subjects were young women and as the experiments were by preference made upon good visualizers, he was not inclined to lay much stress upon these conditions. The phantasm was usually preceded by cloudiness, flushes of color or of light in the medium, and varied from a dim, colorless outline to a fully developed and brilliantly colored picture. The images were

frequently drawn from the patient's recent visual experience, were sometimes fantastic and frequently unrecognised. The successive images were usually associated, if at all, by similarity, but frequently no relation could be discovered between them. Association by contiguity was excessively rare. The phantasm was frequently, but not always, destroyed by movements of the medium and by distracting sensory impressions and motor effort. Occasionally the phantasm was to a considerable degree independent of the medium, persisted for some time after the removal of the medium, and in one such case appeared to obey the laws of the after-image. The importance of such phenomena upon the question as to the value of the central component in the after-image is obvious.

No trace was observed of telepathic or other supposed supernatural agency. There seemed to be no reason for regarding the phantasms of the glass as any thing other than illusions of the ordinary types depending upon the glass as a *point de repère*. Their chief speculative importance, apart from the light which they may throw upon the after-image, lies in the fact that they present to us processes of association by similarity in concrete, sensible form, and in their possible relation to sub-conscious "automatic" processes. While the phantasms as such cannot be regarded as demonstrating the existence of such processes, it is probable that, if sub-conscious automatism exists, its products may be traceable in the phantasms of the glass. It is possible also that some specific relation exists between the hypnotic consciousness and the phantasm of the glass. Dr. Newbold found that images unrecognised by the waking consciousness were sometimes recollected by the patient when hypnotized, and, *vice versa*, experiments by Mr. F. W. H. Myers have shown that a tale related in hypnosis is sometimes presented in the glass externalise in dramatic form.

(15) *Experiments on Dermal Pain.* By HAROLD GRIFFING, Ph. D., Columbia College.

By means of an algometer transmitting pressure up to 15 kilog. the average pain threshold was found to be for forty college students, 5.5; for thirty-eight law students, 7.8; for ninety-eight women, 3.6; for fifty boys, twelve to fifteen years of age, 4.8. The palm of the hand was the place of stimulation. The most sensitive parts of the body are those where the skin is not separated from the bone by muscular and other tissues.

In eighty experiments on two observers the area was variable, areas of 10 mm., 30 mm., 90 mm. and 270 mm. being given. The corresponding average values of the pain threshold were 1.4 kilog., 2.8 kilog., 4.4 kilog. and 6.6 kilog. Thus the pain threshold increases with the area of stimulation, but much more slowly than in direct proportion.

The time in which dermal stimuli of different intensities cause pain was found by noting the time that elapsed before the appearance of pain after weights had been placed in a balance pan in such a way as to press upon the hand. The averages in seconds, based upon eighty experiments on two observers, are as follows: For 100 g., 230 secs.; for 200 g., 35 secs.; for 300 g., 10 secs.; for 500 g., 4.5 secs. Thus the time, as well as the area and intensity of stimulation, are factors in dermal pain. There is, moreover, an intensive limit below which pressure stimuli never cause pain. Above this limit the sensory effect of the time seems to be in direct proportion to that of intensity.

The pain threshold for falling weights was found to depend as much upon the height as the mass. As both the height and mass

are proportional to the kinetic energy of the moving mass, the stimulus for dermal pain in impact must be considered the energy of the striking object.

- (16) *The Normal Night-Blindness of the Fovea.* By CHRISTINE LADD FRANKLIN, Baltimore.

König's announcement in May, 1894, of the very close co-incidence of the curve showing the distribution of brightness along the spectrum for (1) the totally color-blind, and (2) the normal eye in a faint light, with the curve of relative absorption of different portions of the spectrum by the visual purple (and the obvious inference therefrom that the vision of the totally color-blind and that of the normal eye in a faint light are conditioned by the presence of the visual purple in the retina) made necessary some assumption to take account of the fact that no visual purple has hitherto been found in the fovea. Two assumptions were possible,—either that the cones (and hence the fovea) do contain visual purple, but of such an extremely decomposable character that it can never be detected objectively, or that the eye of the totally color-blind person, and the normal eye in a faint light, are actually blind in the fovea. As I had already made the prediction that total color-blindness consists of a defective development of the cones of the retina (*Ztsch. f. Psych. u. Phys. der Sinnesorgane*, Bd. IV, 1892) and also that the adaptation which renders vision possible after twenty minutes in a faint light is conditioned by the growth of the visual purple (*Mind*, N. S. III, p. 103)—both predictions being naturally suggested by my theory of light sensation. I was most anxious to put the latter assumption to the test, I therefore undertook to determine, in the dark room of Prof. König's laboratory, the threshold for light sensation for different parts of the retina and for different kinds of monochromatic light (the full results of this investigation will appear later). The blindness of the fovea for faint light did not at once reveal itself; the act of fixation means holding the eye so that an image falls on the part of the retina best adapted for seeing it, and hence it would involve keeping the image out of the fovea in a faint light, if the fovea were really blind in a faint light. But after the total disappearance of the small bright object looked at had several times occurred by accident it became possible to execute the motion of the eye necessary to secure it at pleasure. It was then found that the simple device of presenting a group of small bright objects to the eye of the observer was sufficient to demonstrate the "normal night-blindness of the fovea" (as it may best be called), without any difficulty—one or the other of them is sure to fall into the dark hole of the fovea by accident. It was only by means of this arrangement of a number of small bright spots that the total blindness in the fovea of the totally color-blind boy could be detected—he had, of course, learned not to use his fovea in fixation. Prof. König then proceeded to demonstrate the total blindness in the fovea of the normal eye to blue light of wave-length about λ 470.¹ [These experiments upon the normal eye were exhibited at Princeton.] It was shown that König's proof that the pigment epithelium is the only layer of the retina which is affected by red, yellow and green light is not wholly conclusive. The interpretation of the new facts, and their bearing upon the several theories of light-sensation were discussed. [This paper will appear in full in the *Psychological Review* for March, 1895.]

¹Prof. v. Kries is said to have shown that the experiments in question do not establish the blue-blindness of the fovea (*Berichte der Naturforschenden Gesellschaft zu Freiburg*, IX, 2, S. 61). I have not yet had access to this criticism.

- (17) *The Muscular Sense and its Location in the Brain Cortex.* By PROF. M. ALLEN STARR.

[This paper was presented in the absence of Prof. Starr. It may be found in full in the number of the *Psychological Review* for January, 1895.]

- (18) *Psychology in the University of Toronto.* By PROF. J. G. HUME, University of Toronto.

In the University of Toronto, we begin the work in psychology, etc., in the Sophomore year. Up to that time the students are engaged in language studies, Mathematics, English History, Chemistry, Biology, etc. After the Sophomore year, they still continue some of this language study as supplemental to the philosophical course. The latter (beginning with Psychology, Logic and Theory of Knowledge in the second year; Psychology, Logic, Theory of Ethics, History of Ethics and History of Philosophy in the third year) keeps extending until in the fourth year those who have selected this course give all their time to the subjects of the course without any supplemental work, taking in the fourth year, Psychology, Ethics, History of Philosophy, Special Reading in the original of various selections from the whole period of modern philosophy, giving special attention to Kant and Lotze.

In Experimental Psychology: Second year, second part of the year: Demonstrations from the director, explanations of methods and practice. In the third year during the whole year, the class divided into groups, is under the charge of the director in the laboratory. In the fourth year, they are supposed to be able to undertake experiments of an independent character. Some of the enquiries started in the fourth year are continued in post-graduate work. In the present fourth year, there are sixteen honor students, conducting four sets of experiments, that is in four groups with four in each group: I. On Time-reactions (mechanical registration instead of the chronoscope). II. Discrimination of Geometrical Figures and Letters in the Field of *Indirect Vision*. III. Discrimination of Color-Saturation. IV. Discrimination and Reproduction of Rhythmic Intervals. In post-graduate study, there are two enquiries being continued from last year: I. Estimation of Surface-Magnitude. II. On Certain Optical Illusions. The director of laboratory, Dr. August Kirschmann, has in the press a recently finished investigation upon the nature of the perception of *metallic lustre*. [This account was presented in the absence of Prof. Hume.]

PSYCHOLOGICAL LITERATURE.

I.—NEUROLOGICAL AND COMPARATIVE.

By C. F. HODGE, Ph. D.

The Insanity of Over-exertion of the Brain. The Morrison Lectures before the Royal College of Physicians of Edinburgh, 1894. J. BATTY TUKE; pp. 66, 9 Figs.

"To approach the treatment of the insanities through the portal of psychology is hopeless; we have gained nothing by taking that road in the past, and can hope for nothing in the future." Not only has nothing been gained, but great harm has been done and the whole subject of the cause and treatment of insanity has been burdened with serious misconceptions which it must take a long time to outgrow. Although these words appear late in the book, they form the basis of the author's treatment of the subject. The only hope lies in approaching insanity from the side of anatomy and physiology. The organ concerned is primarily the brain. Chapter I is therefore devoted to a description of a cerebral convolution, and here we find the most recent results of the Golgi-Cajal methods utilized to the fullest extent. Not only nervous structures but the membranes and the connective tissues, the blood and lymph mechanisms are clearly brought out. Every efficient physician forms mental pictures of the condition of the organ he is called upon to treat. These for the brain specialist must be as clear and as true to nature as it has long been attempted to make them in case of disease of kidney or heart—as perfect as our knowledge of brain anatomy and histology can give.

Chapter II follows logically with the recent advances in our knowledge of brain or nerve physiology. It is in this field that the author finds, "Causes of Implication of the Apparatus." Intra-cranial circulation is first discussed both as to quantity and quality of circulating blood and lymph. Here Mosso's work naturally comes into prominence in connection with the hyperæmia during mental activity, and among that of others the anæmia of sleep. These facts are coupled with the further discoveries of Mosso that brain work weakens the muscles. If blood from a fatigued animal is transfused into one that rested, the latter shows every sign of fatigue. This proves that activity must give rise to products of decomposition which affect the organism as poisons. One possible source of such products is indicated by the change in nerve cells during experimental or daily fatigue, as recently brought out by Hodge. In fact, as the title implies, the author would maintain that in general, insanity always has its beginning in "Over-exertion of the Brain" and its consequent fatigue. Eight of the author's figures are given to enforce this point most clearly; and these are all photographic reproductions made from sections of cat's spinal ganglion cells and honeybee brains, morning and night, furnished to Dr. Batty Tuke from

the neurological laboratory of Clark University. While fatigue is within physiological limits, although extreme, a comparatively rapid recovery may occur. Under this head Dr. Batty Tuke takes occasion to remark, and without doubt truly, that the fatigue in Hodge's experiments, in which the effects of five hours stimulation required twenty-four hours complete rest, had overstepped the normal. Possibly for the ganglion cells of a cat this is true. Still even more pronounced evidences of cell-fatigue can be demonstrated in birds and honeybees at night, and these probably do not exceed the normal. Recovery in these animals undoubtedly takes place much more rapidly.

The area of the cortex found to present changes similar to those pointed out in fatigued nerve cells, is generally confined to the central region, and the large pyramids are first to show a change from the normal. Connected with the hyperæmia which accompanies excessive fatigue we find over this area the pia edematous, thickened and milky, and the Pacchionian villi often hypertrophied. Contrary to the opinion of many on the subject, Batty Tuke does not believe that any other disease, or the disease of any other organ can act directly as the cause of permanent insanity. Even their indirect influence in worry, pain, loss of sleep, etc., he would limit more strictly than is usually done, and cites in support of his view that many of the most painful and distressing diseases, calculus, fistula, rectal or uterine cancer, stricture, etc., are not specially inimical to brain health. Between extreme conditions of fatigue prostration and insanity it is often hard to distinguish, so hard, in fact, that it is frequently said to be impossible, that sanity and insanity shade into one another by imperceptible gradations. We are glad to have stated by so high an authority that "between the two" (normal and insane conditions), "there is a distinct line of demarcation. This he finds in the individual's reaction to external circumstances. As soon as external impressions begin to lose their influence in determining judgements, normal wear has been exceeded and the mechanism of sensation or association has become seriously impaired."

The most hopeful part of the book is the line of treatment laid down for cases of over-exertion-insanity. Foreign travel, change of scene, etc., so often recommended are as clearly contra-indicated as mountain-climbing would be in pneumonia. Not change of stimuli but cessation of all stimuli is demanded. Rest, rest in bed, should be insisted upon. The patient should be carefully secluded. Possibly the other members of the family may be required to leave the house. Sleep must be promoted by all natural methods, hygienic, dietary and by massage. With Dr. Cowles, the author insists that in conditions of the brain suffering from over-strain drugs are too apt to do harm and delay recovery. They should be used only as a last resort, in any case. In hypnotism, Batty Tuke has no faith. Treatment for from three to six weeks are necessary to produce convalescence and one to three months thereafter for complete recovery. It is during this period that recreation and change may prove beneficial. To make the method of treatment available for the poorer classes each city hospital should have a ward set apart for such cases. Patients should be admitted on a physician's recommendation, and a few week's care at the right time might in no small number of cases restore a man to his work and save the state the expense of years of asylum treatment. No one can have read Dr. Cowles' book on *Neurasthenia* without being impressed by the generally hopeful convergence of view that it is possible to prevent the development of insanity, if nerve-fatigue is recognized and treated before it is too late.

There is but one thing more to be desired and that is that we gain and rationally apply to the conduct of daily life a sufficient knowledge of brain physiology to guard even against an amount of over-exertion which may not be wholly recovered from by the sleep of a single night.

Eine neue Theorie über die Ursachen einiger Nervenkrankheiten, insbesondere der Neuritis und der Tabes. L. EDINGER. Leipzig, 1894. pp. 116.

This paper is one of a series of clinical lectures and consists chiefly of citations of clinical points taken from the daily experience of the author during the last three years. Its main interest is to be found in its general agreement, although covering a much wider field of nervous diseases, with the general trend of Batty Tuke's argument. All nervous diseases must be referred to functional rather than to structural causes. They can all be explained by Edinger upon the theory that nutritional regeneration fails to equal the destruction of substance occurring in the course of functional activity. One and the same exciting cause anæmia, syphilis, et. al. will thus produce disease in that part of the nervous system which is most severely taxed by the work of the individual. In officers, railroad employees and foresters we have tabes of the lower extremities; while from the same general physical condition we find cases of progressive paralysis cropping out in those who are engaged in mental work.

Untersuchungen über den feineren Bau des centralen und peripherischen Nervensystems. CAMILLO GOLGI. Translated from the Italian by Dr. R. Teuscher. Gustav Fischer, Jena, 1894. Quarto 273 pp. 30 quarto plates.

An epoch in the knowledge of the nervous system was marked by the appearance in 1885 of Golgi's book, "Studi sulla fina Anatomia degli Organi Centrali del Sistema Nervoso." The Italian edition has long been exhausted, as some of us have occasion to know who have had orders for the book placed for four or five years. The present fine edition of Fischer's will thus fill a long felt need. In it we have Golgi's most important communications between 1871 and 1893. His figures and descriptions of nerve cells with their processes, protoplasmic and nervous, have become so familiar that they need no explanation. But since Golgi first outlined his main positions a number of questions have come to be of immense importance to neurologists and we naturally turn to this latest edition of his works to find his present position clearly stated. Possibly first comes the question: What is the function of nervous and protoplasmic processes? Long before Golgi's work and the discovery of his staining method, histologists had recognized a difference between the processes which arise from the body of a nerve cell; but the methods of Deiters, Gerlach and others failed to demonstrate these to any great length. Moreover M. Schultze demonstrated with apparent clearness that the structure of both axis-cylinder and protoplasmic processes are alike in possessing ultimate nerve fibrils, and that these fibrils may enter a cell by one process and pass out by another without any branching or break of continuity in the cell. The very natural supposition then arose that the cells were connected by their protoplasmic processes, dendrons, with one another and either received sensory impressions or discharged motor impulses through their neurons, which were then supposed to be unbranched. A reflex arc might thus consist of the following parts: First, a sensory neuron, entering the cord through the dorsal root and passing to its sensory cell; second, the dendrons of this cell connecting with those of a motor cell; and third, a neuron passing from a motor cell to a muscle. Golgi succeeded in following out these processes much further than former histologists, to what would seem to be their ultimate terminations, and in no case did they unite with the dendrons of other cells. They did, however, show a general tendency to grow out towards the blood vessels and glia cells in the neighborhood and this fact led Golgi to advance the theory that dendrons are closely connected with the nutrition of the nerve cell. This position Golgi finds no reason to modify. The evidence which has been brought to bear

upon this point chiefly by Cajal and Van Gehuchten is, according to Golgi's view, purely theoretical and is not derived from any new facts discovered by them in nerve histology. The answer to this question is to be found where the reader is least likely to look for it, viz., in the last chapter and upon almost the last page of the book. This chapter is entitled, "Upon the origin of the fourth cranial nerve and a general question of cellular physiology which is connected with it." The particular point here is that the cells which give origin to the fourth nerve have a single neuron and no dendrons whatever. If on Cajal's hypothesis, that the dendrons are the organs of the nerve cells by which impulses are received and that the neuron furnishes the path for the discharge of the nerve impulse, how do cells like these having no trace of dendrons, receive stimuli? In order to bring the cells of the spinal ganglia into his system, Cajal has been obliged to suppose that the neuron to the skin is in reality to be considered a dendron. This, according to Golgi, is seriously straining facts to make them agree with theory.

The second important question upon which we desire to have Golgi's present opinion touches the relation of nerve cells to one another. Do their processes actually unite or do they merely come into contact? Golgi replies to this question with an entire chapter describing "The diffuse nervous network of the central organs of the nervous system and its physiological significance." In his former book he advances the view that the branches of the neurons unite to form a close-meshed network throughout the entire central gray matter. The great complexity of this structure made difficult the demonstration of actual union of processes from different cells; but Golgi now claims to have made preparations which leave no room for doubt. The contact theory has been so ably advocated of late by Ramon y Cajal, Kölliker, Van Gehuchten and others that this word from Golgi is most opportune. Golgi also insists more strongly than ever, if that is possible, upon the characteristic difference between neurons and dendrons, and to the objection of Obersteiner, that the Golgi method does not enable us to distinguish with certainty between these two kinds of processes, Golgi replies that this only proves that Obersteiner has never been able to obtain good preparations.

The bearing of Golgi's view on his conception of cerebral localization may be gathered from the emphasis which he places upon the fact that we have absolutely no subdivision of the cerebral cortex corresponding to the so-called "centres" of the localization school. No anatomical divisions exist, and in histological character the entire cortex is of essentially the same structure. In this there is no denial of a certain degree of localization. Regions, not sharply defined, into which a nerve enters directly or from which it most immediately springs, are naturally more distinctly concerned with its special function. But the presence of a diffuse nervous felt-work including the entire central gray matter must tend to bring us back toward something like the old position of Flourens, viz., that the entire brain, being a unit in structure, is also a unit in function.

Ueber ein neues Eintheilungsprincip der Grosshirnoberfläche. P. FLECHSIG.
Neurologisches Centralblatt, XIII, p. 674, Leipzig, 1894.

The new division of the cerebral surfaces suggested by Professor Flechsig is the natural result of his long and eminently successful studies upon fiber systems in the brain and the order of their development in the child and human embryo. By these fiber-systems the cerebral hemispheres may be divided into two grand divisions. The first includes those areas which receive, or give origin to sensory or motor fibers (the sensory and motor areas of the localizationalists) besides a few

association fibres. The second great division of the brain has no direct connection whatever with the corona radiata, but contains only association fibres. For sake of brevity Flechsig designates the first class of areas as sensory centres, "Sinnescentren," and they include the optic area around the calcarine fissure, the auditory area in the posterior part of the first temporal convolution, the olfactory area in the hippocampal gyrus and the posterior part of the inferior surface of the frontal lobe, and last the great central motor region about the fissure of Rolando, including the posterior portions of the frontal convolutions.

The second great class of areas, the association centres, "Associationscentren," occupy the four great tracts, terra incognita, not accounted for by the localizationalists. These are the anterior portion of the frontal lobe, the island, a large part of the temporal lobe, and a large region in the parieto-occipital lobe, including præcuneus and the posterior portion of the parietal lobe. The extent of these areas can best be determined in the brain of a three-months old child. At this age almost the entire corona radiata is medulated, and these streams of medulated fibres spray out to distribute themselves solely in the sensory centres above described. Scarcely one-third of the cortex is thus supplied with medulated nerve fibres, and the large association areas comprising more than two-thirds of the entire brain surface are either entirely destitute of medulated fibres or contain only a few scattering fibres which come to them, for the most part, from the sensory centres. It would thus seem at this stage of development each sensory centre possessed its own sensory mechanism distinct from every other. Later, at what age it is not stated, each association centre develops association fibres which unite it with two or more sensory centres, and these fibres are much more numerous than fibres of association which unite sensory with sensory centres. The greatest difference, according to Professor Flechsig, between the brain of man and that of other animals is found in the enormous development of the association centres. Their development, in fact, determines the type of brain and the form of the skull.

Beitrag zur Lehre von der absteigenden Degeneration in Gehirn und Rückenmark und Bemerkungen über die Localization und die Leitungsbahnen der krämpfe bei der "Absynth-Epilepsie," ROBERT BOYCE. *Neurologisches Centralblatt*, Bd. XIII, p. 466, 5 figs.

Boyce has carried out a long series of experiments upon cats to determine first, the exact descending degenerations connected with the different operations, and, second, the locus, or loci, of origin for the convulsions in, "absynth-epilepsie." The following observations were made: 1. Extirpation of motor areas of one side. This is followed by degeneration of the corresponding pyramid, no other tract being implicated in the least. 2. Extirpation of one hemisphere, or, what amounts to the same thing, hemisection of the mid-brain. After this operation degeneration occurred in the descending root of the fifth nerve, in the posterior longitudinal bundle, both on the same side, and Meynert's and Forel's bundles of the opposite side. These degenerations were studied by Marchi's method and are made very clear by a well selected series of drawings.

Either directly after the operation, or after the animal had recovered, absynth was injected and a record was obtained upon the myograph from the extensor muscles of both fore legs. Asymmetrical epileptiform cramps were found to occur. If contractions had been wholly absent from the side corresponding to extirpation of the motor areas, this would have proved that these areas are the sole loci of origin for the convulsions. It was found, however, that centres exist in the medulla and cerebellum which are capable of originating epileptiform cramps of the typical clonic character. The rhythm is, however, much slower than

upon the uninjured side. Hemisection of the cervical cord stops all contractions upon the operated side, which proves that it is not possible to stimulate the cord in this way. In this connection it will be remembered that Goltz notes the occurrence of epilepsy in dogs from which the cerebrum has been removed, and this without the use of absynth. The most valuable result of Boyce's experiments lies in demonstration of the fact that epilepsy may be looked upon as a reaction of certain centres in the brain to a poison which may pervade the whole system. Its maximal affect is produced, when the cerebral cortex is intact; but centres in the medulla and cerebellum are sufficiently sensitive to be affected. Many cases of epilepsy in man are doubtless due to similar sorts of intoxications, and the fact that the convulsions begin in centres of irritation, *i. e.*, foci of highest sensitiveness, is further support for the generally accepted views. It would seem reasonable, however, that treatment should begin with the toxic substances in the blood rather than with extirpation of sensitive parts of the brain.

A Microscopical Study of the Nerve Cell during Electrical Stimulation. C. F. HODGE. *Journal of Morphology*, Vol. IX, pp. 449-463, 5 Figs. Boston, 1894.

Changes in Ganglion Cells from Birth to Senile Death; Man and Honey-Bee. C. F. HODGE. *Journal of Physiology*, Vol. XVII, pp. 129-135, Plate IV. Cambridge, 1894.

Die Nervenzelle bei der Geburt und beim Tode an Altersschwäche. C. F. HODGE. *Anatomischer Anzeiger*, Band IX, Su. 706-710, 4 Figs.

The first of the above papers forms a new chapter in the nerve-cell-fatigue-work, reports of which have been given in this JOURNAL since 1889. By means of specially devised apparatus the spinal or sympathetic ganglion cells taken from the same frog were kept for different lengths of time in a gentle stream of salt solution upon the stages of two similar microscopes. Comparable cells were sought out in each preparation and electrical stimulation was then applied to the one and not to the other, and drawings, by means of the camera lucida, as well as careful measurements, were made of both preparations at regular intervals. Thirty-three experiments were made in all with the fairly uniform result that the nucleus could be seen to gradually shrink in the cells to which stimulation was applied. This decrease in size may amount to as much as 58% in twenty minutes but never exceeded a loss of 75%. The cell as a whole did not shrink perceptibly, but after treating with osmic acid the stimulated cells could be seen to be pervaded by irregular light spaces, representing probably the vacuoles figured and described in former papers. The greatest shrinkage of the nucleus observed in the control cells was 19%. Experiment 3 continued for six days, during the whole of which time it was possible to distinguish nuclei, nucleoli and the granulation of the cell-protoplasm. Active changes however, in the nucleus ceased to be discernable after six hours. Curves of nerve-cell fatigue obtained by plotting the shrinkage of nuclei differ somewhat from the curves which were formerly derived from cells while in the body. In the latter case the nuclei shrank rapidly at first, then very slowly, or gained a little, and finally decreased in size quite rapidly again to a condition of apparent complete fatigue. When the cells are removed from the body and placed in a non-nutrient solution, as might be expected, no such intermediate recovery occurs. Thus curves derived from measurements of nuclei during stimulation show a much more rapid decline than in case of cells in contact with their normal nutritive supply in the body. They come, in fact, to closely resemble fatigue curves for an excited muscle. With a stream of saline solu-

tion continually bathing the cells, it is difficult to conceive that fatigue in this case is to any extent due to accumulation of decomposition products. It was possible also to observe changes in the nucleolus. In general this decreased in size during stimulation, and the reason for this shrinkage could be seen by close observation to lie in the fact that granules were extruded from the nucleolus into the nucleus. If a little potassium tartrate, 0.1% be added to Ringer's solution, the nucleoli in the stimulated cells undergo active, apparently amoeboid, changes of form and move about from place to place in the nucleus. They very soon fragment, however, and dissolve. By this method changes resembling those occurring in fatigue can be demonstrated much more quickly than with stimulation of the ganglia while in the living body.

The last paper is an abstract of the second with addition of four figures in the text. The purpose in this investigation is to determine so far as possible the characteristic differences between young and old nerve cells. Especially good material for demonstrating the extreme phases in the process of ageing was supplied by portions of the nervous system from a mandy at the age of ninety-two and of old age apparently uncomplicated by any disease, and to compare with these similar preparations from a male foetus, killed by accident of birth. The brains of twenty-one old bees were also compared with the same number of young bees' brains. They were in every case caught as they emerged from the brood cell and the old bees were selected by age signs, abraded hairs and frayed wings, etc.

In the cerebrum of the old man no abnormality has been as yet detected by methods thus far employed. The cells appear normal in size and number, so far as this can be determined without special counting, and the nuclei and the nucleoli are in all respects normal. The cells of Purkinje were 25% fewer by count in the cerebellar cortex than in a similar preparation from the cerebellum of a man killed by accident at the age of forty-seven but this difference may be nothing abnormal. Both protoplasm and nucleus of these cells also appear considerably shrunken. The most marked abnormality was found in the cells of the spinal ganglia. Perhaps the most important difference here between young and old cells is a failure of the nucleoli in the old cells to stain with osmic acid. The nuclei, also, in the old cells are considerably shrunken and present irregular outlines and the cell protoplasm is filled with pigment. These differences may be most readily gathered from the following table.

	Volume of nucleus.	Nucleoli observable in nuclei.	Pigment much.	Pigment little.
Foetus	100%	53%	0%	—
Old Man	64.2%	5%	67%	33%

Nucleoli, comparable to those of vertebrate nerve cells, are not present in the bee's brain, nor is anything resembling the pigment granules of vertebrates to be seen. Aside from these features, however, comparison of young and old bees yields results quite similar to those obtained from human material. The cell protoplasm in the old bees is much vacuolated and the nuclei are shrunken, often almost beyond recognition. In all the young bees the protoplasm is dense and the nuclei are so large in proportion to the size of the cells that they often are pressed into polyhedral forms. In all the sections, one is also impressed by the much greater number of cells in the young brains. An actual count of comparable groups of cells gives one cell in the old to 2.9 cells in the young. Age changes in nerve cells are so marked and in so many respects resemble the changes produced by fatigue that in making experiments on fatigue, the age of animals compared should be taken into account.

Histological Changes Induced in Sympathetic, Motor and Sensory Nerve Cells by Functional Activity. GUSTAV MANN. *Journal of Anatomy and Physiology*, Vol. XXIX, pp. 100-108, Pl. I.

Dr. Mann's experiments as described in the present paper deal with changes occurring in sympathetic ganglion cells of the rabbit caused by electrical stimulation of from thirty minutes to nine hours; with the retina and occipital lobes of dogs, in which one eye had been exposed to light, while the other remained covered, and with the motor areas and motor cells in the spinal cord of dogs after considerable fatigue and in the resting condition. The method yielding most definite results are methyl-blue or toluidin-bluestaining after hardening in mercuric chloride. The author's conclusions are as follows: 1. "That during rest, several chromatic materials are stored up in the nerve cell, and that these materials are used up by it during the performance of its function." 2. "That activity is accompanied by an increase in size of the cells, the nuclei and the nucleoli of sympathetic, ordinary motor and sensory ganglion cells." 3. "That fatigue of the nerve cell is accompanied by shrivelling of the nucleus and probably also of the cell, and by the formation of a diffuse chromatic material in the nucleus. As far as our author's fatigue changes are described, there is a rather close agreement with results of my own experiments. It is further quite possible that there should be an initial swelling of the cell on beginning stimulation, due, as Dr. Mann suggests, to a flow of lymph into the cell. My experiments were directed chiefly to the demonstration of extreme fatigue and do not cover this point.

The Microscopical Examination of the Human Brain. EDWIN GOODALL, M. D. London, 1894. pp. 186.

We have here presented in a clear concise manner several hundred methods for the microscopical preparation of the brain. The book is at once a compendium for ready reference to all sorts of devices of treatment and a critical statement derived from experience as to the practical value, difficulties, etc., of each method. It is English, and of course we are treated on the same page with formulae calling for drachms and ounces, and grammes and kilos. The metric system is followed in the main however. An appendix of fifty pages is devoted to museum methods.

Schema vom Faserverlauf im Rückenmark. E. VILLIGER. Basel, 1894. 19 pp. with large colored diagram.

In the preparation of the above schema Villiger has made full use of recent results of v. Lenhossék, Pierre Marie, Ramon y Cajal, Déjérine, L. Edinger, and A. Strümpell. The result is a convenient diagram, drawn in perspective, giving all the different kinds of cells, including those of the spinal ganglia with the course of their respective neurons within the cord. Each type of cell has its own color and this is continued into the neurons arising from it. The normal direction of the nerve impulse in each fibre is also indicated with the direction in which degeneration occurs after injury. The plate is almost as good as a model.

Zur Physiologie des Labyrinths, Mittheilung; Das Hören der labyrinthlosen Tauben. J. R. EWALD. *Pflügers Archiv* Bd. LIX, Sn. 258-275. Bonn, 1894.

By over ten years active experience upon this line of work Prof. Ewald has arrived at a degree of skill which has made him complete master of his difficult and extremely delicate operations. The writer is personally under great obligation to Professor Ewald for a most careful and thorough demonstration of not only the operated animals but of his method of removing the membranous labyrinth. Objections have been raised (by Bernstein and Matte) against Ewald's conclusions, on

the ground that portions of the labyrinth were overlooked in the extirpation, that these parts remaining intact might continue to mediate some degree of audition. The writer is glad of an opportunity to return Prof. Ewald's great courtesy by recording the testimony that any oversight of the kind indicated is out of the question. Without the shedding of a single drop of blood to obscure the anatomy of the parts in the least the entire labyrinth was removed and writer could plainly see the empty bony capsule and at Prof. Ewald's request actually counted the five stumps of the branches of the auditory nerve as they had been torn from their end-organs. There is no possibility of any doubt as to completeness of extirpation. Passing next to the operated animals, there is certainly no more possibility for doubting that they react to certain sounds than that a normal dog reacts to a whistle. The fact that Matte's pigeons did not respond to the report of a percussion cap, and this was the only test according to Ewald that Matte had recourse to, proves little or nothing; since normal birds seldom react to a noise of this kind. The writer has often shot wild pigeons one after the other out of a tree without the remaining birds paying any attention to the report of a gun. We are thus confronted by the fact that animals, wholly deprived of auditory sense organs, are still able to react to certain sounds. Possibility of contact with anything which could vibrate, the method so well known by which the deaf are enabled to appreciate sound vibrations, was guarded against by suspending the cage from the ceiling and supporting the pigeon on a pad of cotton several centimeters thick. The sound is now conducted through a rubber tube twelve feet long from the mouth of the observer to within 10 cm. of the pigeon. The sound which Ewald generally used is a long drawn "Uh" made during inspiration. Disturbance of the air in the neighborhood of the bird would thus be exceedingly slight. In fact, quickly drawing a full inspiration through the tube never caused any response on the part of the pigeon. In his book on the eighth nerve Ewald advances the theory that the stumps of the nerves are capable of being stimulated by sound vibrations. He has now applied arsenical paste to kill the nerves but is still unable to obtain a bird which does not react to sound. For the present, therefore, the author contents himself with a statement of the facts and does not attempt to frame any theory as to the mechanism by which his birds are enabled to appreciate sound vibrations.

Zur Frage nach der Vererbung erworbener Eigenschaften. REH. Biologisches Centralblatt, XIV. Bd. (1894), s. 71-75.

The author, after referring to the views of Häckel, Darwin, Weismann, Haacke, and stating his belief that the doctrines of Häckel and his followers are not absolutely antagonistic to those of the school of Weismann, expresses his own view as follows: The question is not one of the inheritance of "operative mutilations," but of "acquired characters." This undoubtedly exists, but it presupposes a fixed *Anlage*, innate (*innewohnend*) and given by the systematic position. To have shown this, is the great merit of Weismann.

A. F. C.

Untersuchungen über die Folgen der Zucht in engster Blutverwandtschaft. DR. RITZEMA BOS. Biologisches Centralblatt, XIV. Bd. (1894), s. 75-81.

As the result of experimental breeding of the descendants of a tame albino female rat from October, 1886, to 1893, Dr. Bos concludes: 1. Continual interbreeding of close relations decreases the power of reproduction, and may even cause at last complete infertility. 2. It appears, also, after many generations to induce a

decrease of size of body. 3. It is possible, but in no way proved, that the continual interbreeding of close relations causes a greater predisposition for diseases and the occurrence of malformations.

A. F. C.

II.—ANTHROPOLOGICAL PSYCHOLOGY.

By A. F. CHAMBERLAIN, Ph. D.

Suicide among Primitive Peoples. S. R. STEINMETZ. Amer. Anthropol. (Washington), Vol. VII (1894) pp. 53-60.

It has been assumed by many authorities that insanity and suicide increase in the ratio of the civilization of the races. In this article the author of the excellent "*Ethnologische Studien zur ersten Entwicklung der Strafe*," gives us the result of his examination of the literature relating to primitive peoples in the matter of suicide. "It seems probable from the data I have been able to collect that there is a greater propensity to suicide among savage than among civilized peoples, and that its frequency may be owing to the generally more positive faith in the future life existing in the former races which enables them to meet death with greater calmness and a slighter resistance of the instinct and other natural motives tending to conservation of life, and finally the question suggests itself that if suicide is one of the positive symptoms of moral degeneration, as Dr. Winkler suggests, is it possible that moral degeneration is taking place among the primitive peoples?" The motives leading to suicide are generally the same as those active in all civilized societies, a fact which controverts the opinion of Morselli.

African Fetishism. CHATELAIN HELL. Journ. Amer. Folk-Lore. Vol. VII (1894), pp. 303-304.

This is a clear statement, in brief terms, by one who can speak with authority on the subject of African religion. The author's conclusion is worth reproducing here. "The more I ascertain and compare original facts, the more am I impressed with the fundamental unity of the religious conceptions of Chinese, Hindoos, and American Indians, as well as of nominal Moslems, Jews and Christians, with the African negro. They all have a dim notion of a supreme being; they all serve him far less than they serve the spirits, the mysterious forces of nature, and the souls of deceased persons (ancestor worship, etc.), and put their trust in amulets, talismans, incantations, quacks, priests, soothsayers, spirits, and the thousand and one manifestations and paraphernalia of the one universal disposition of mankind known as superstition."

African Races. CHATELAIN HELL. Journ. of Amer. Folk-Lore, Vol. VII (1894), pp. 289-302.

After all the books and magazine articles on the "dark continent" this essay comes with refreshing simpleness of statement and lack of racial bias or theoretic askewness. The author, and his researches entitle his opinion to the greatest respect, holds a much higher opinion of the African negro than is wont to be entertained in psychological and anthropological circles, and he is probably right in so doing. Interesting to the psychologist is Mr. Chatelain's declaration: "The four main causes of the cultural inferiority and of the miseries of the African negro's life can be reduced to four heads namely, first, the lack of a written literature; second, the institution of polygamy; third, that of slavery; fourth, and chiefest, the belief in witchcraft. The development of the race and the happiness of the individual depend on the healing of these sores." The author evidently anticipates the adoption

of Christianity by the negro, with a native literature, and the development of a great negro civilization, for he scouts the idea of any final spoliation of the continent by the whites. Another race of importance from a psychological stand-point is the Hamite, of the western branch of which, the Berbers, who have occupied their present habitat from time immemorial, Mr. Chatelain says: "The great civilization of their Egyptian cousins, the luxury of Carthage, the power of ancient Rome, the fire of Islam, have past by or over them and left them almost unchanged. Never daunted, scarcely influenced, they have, however, adopted Islam but without sacrificing their own individuality. Fierce tribal Independents, they have not even allowed the formation of a national government. Here we have a branch of the white race, naturally the equal of any other, showing no sign of degeneration and from the first in contact with the best civilizations, yet proudly stationary on a level of culture but slightly superior to that of the Central African negro, who for thousands of years has had no civilization within his sight or reach," (p. 294).

Technogeography, or the Relation of the Earth to the Industries of Mankind. O. T. MASON. Amer. Anthropol., Vol. VII (1894), pp. 137-161.

The author of this interesting essay defines technogeography as "the study of the relationship between the earth and human arts and inventions," a sub-division of the broader subject of anthropogeography, "the consideration of the earth in its broad connections with the whole science of man, including his body and his mind, his arts, languages, social structures, philosophies and religions." Prof. Mason proceeds to discuss the earth as the producer of mankind, as a storehouse of materials, as a reservoir of forces, as a teacher of processes, the earth as a whole, as an organized structure, the culture-areas of the earth, the earth as a single culture-area, the earth in relation to the higher artificial life. His paper is a useful contribution to philosophical anthropology.

Migration and the Food-Quest. O. T. MASON. Amer. Anthropol., Vol. VII (1894), pp. 275-292.

This paper, which has as sub-title, "A Study in the Peopling of America," is devoted to a discussion of that problem from the migration-motive of the food-quest. The author "disclaims any reliance upon continents that have disappeared, upon voyages across the profound sea without food or motive, the accidental stranding of junks, or the aimless wandering of lost tribes. When the continent of America was peopled, it was done by men and women purposely engaged in what all sensible people are now doing, namely, trying to get all the enjoyment possible out of life for their efforts." The author is able to see a closer relation between the peoples of America and those of the eastern Asia, than is seen by Brinton and other authorities, but the chief proposition he defends is "this close connection between the two continents has existed for thousands of years, during which the contact between western America and eastern Asia was more and more close, and extended, and unbroken, as we proceed backward in time. Or, to put the matter in another shape, there never was known to history a day when the two continents were not intimately associated."

The Half-Blood Indian, an Anthropometric Study. FRANZ BOAS. Pop. Science Monthly, Vol. XLV (1894), pp. 761-770.

This valuable study is based upon material collected for the department of ethnology of the World's Columbian Exposition, the charge of the section of physical anthropology having been given to Dr. Boas.

The principal facts disclosed by the investigations, of which the author gives a brief summary are : (1) the mixed race is more fertile than the pure stock, contrary to the opinion generally entertained regarding hybrid races ; (2) the statures of Indians and half-bloods show differences which are in favor of the half-bloods. The latter are almost invariably taller than the former, the difference being more pronounced among men than among women. The white parents of the mixed race are mostly of French extraction, and their statures are on an average shorter than those of the Indians ; (3) the facial measurements of the half-bloods are intermediate, the average value being nearer the typical Indian measurement, and remote from the white measurement ; (4) the half-blood has a narrower nose than that of the Indian, with thinner alæ ; (5) the measurements of length of head of the Ojibwa and métis show a gradual increase in length from the full-blood, through the three-quarter-blood to the half-blood.

The Anthropology of the North American Indian. FRANZ BOAS. Mem. Intern. Cong. Anthropol. (Chicago, 1894), 1893, pp. 37-49.

This is a concise account of the general results of the measurement of some 17,000 full-blood and half-breed Indians from all over the North American continent, with the exception of the Arctic coast and the Mackenzie basin. The facts brought out of greatest importance are : (1) The average number of children of Indian women is high, and therefore, the decrease in their numbers can only be explained by the fact that there exists a very high infant mortality ; (2) on an average the breadth of face of the Indian is 1 cm. more than that of the American white (the latter, however, is exceeding narrow, as compared with that of some Europeans) ; (3) on the whole, the North American Indians may be called a tall people ; (4) in the areas where deformation of the head has not obtained, Dr. Boas recognizes four well characterized types of skull which cannot be combined or considered as closely related ; (a) the mesaticephalic (index approximately 79) population of the whole Mississippi valley ; (2) the long-headed Eskimo of the eastern Arctic coast ; (3) the exceedingly short-headed types of the North Pacific coast, and in isolated spots further down the coast ; (4) the long-headed type of southern California. He is inclined to admit an early contact of the Eskimo and Micmacs to explain the low indices of the latter.

Grundzüge der Anthropologie für höhere Lehranstalten, Lehrer-Seminare und Lehrer, sowie zur Selbstbelehrung für jedermann. A. SPROCKHOFF. Revidiert durch Geh.-Rat Prof. Dr. Rud. Virchow in Berlin. *Der Körper des Menschen. Gliederung, Bau und Thätigkeit seiner Organe mit besonderer Berücksichtigung der Gesundheitslehre, sowie der Krankenpflege und der ersten Hilfe bei Unglücksfällen nach Prof. Dr. von Eschmarch in Kiel.* Zweite, vermehrte und verbesserte Auflage mit 153 instructiven Abbildungen. Hannover, 1892, XXIX, 290, s., 80.

This is an excellent treatise—intended for use in the higher institutions of learning—on anthropology, in the narrower, physical and somatological sense of the term. Brief and clear expositions of the body and its members and organs, their anatomy, physiology and hygiene are given. There is besides a special section (s. 211-266) on the elements of hygiene, and the first procedures in case of sickness and accident. An appendix (s. 267-275) deals with psychic life. The book is provided with a good index and a glossary of scientific (Latin and Greek) anatomical and other terms with their equivalents in German. The ethnological portion of the book (s. 196-210), which is provided with the usual set of race portraits, is the least satisfactory part. The

American Indians are, as usual, classed as Mongols. For a German textbook, however, the work is a vast stride in advance.

The Origin of Sacred Numbers. D. G. BRINTON. Amer. Anthropol., Vol. VII (1894), pp. 168-173.

In this brief paper the author deals with "holy or sacred numbers as observed in the early civilizations, and among tribes living in what we call primitive conditions." His conclusions are: (1) The sacred numbers are preëminently 3 and 4, or derived from these; (2) these numbers represent contrasting or antithetic symbolic notions, and arise from wholly opposite mental perceptions; (3) the number 3 derives its sacredness from abstract subjective operations of the intelligence, and has its main application in the imaginary and non-phenomenal world; (4) the number 4 derives its sacredness from concrete and material relations from external perceptions, and has its application in the objective and phenomenal world; (5) the associations which attach sacredness to these numbers arise in the human mind of the same character everywhere and at all times, so that no theory of borrowing is needed to explain identities or similarities in this respect; (6) ethnic character, however, tends potentially to develop especially the one or the other, either the abstract symbolism or the 3 and its derivatives, or the concrete symbolism of the 4 and its derivatives; and conversely the preponderant development of the one or the other of these reveals, with instructive precision, the ethnic character of tribes and nations. In the "three series," we have the various trinities of time, space, position; creation, preservation, destruction; birth, life, death; three worlds; divine triads—the trinities of Buddhism, Christianity, etc. Derived from 3 are 9 and 33, numbers of significance in Teutonic and Hindu mythology. In the "four series"—derived directly from the relations of the human body to the external world about it—we have the four cardinal points, Janus, the four-faced Roman year-god, and the world-wide occurrence of the number four in myth, ritual and ceremony. Derived from 4 are the numbers 7 and 13, sacred or tabu'd numbers in many lands and among many peoples—the 7 planets; the 7 "ancient spaces" of the Zūfi Indians; the 7 caves of Aztec legend, etc.; the 13 islands of which the earth consists according to Hindu cosmogony; the 13 months of the North Asiatic and primitive Aryan solar year, etc.

As to ethnic facts, Dr. Brinton states that "the American and Mongolian races revere almost exclusively the "four series," for which also the ancient Babylonians had a decided preference; while triads and trilogies are Egyptian and Greek, Teutonic, Celtic, Slavonic, Indic. Dr. Brinton's paper is another valuable addition to the literature of anthropologic psychology.

A Primer of Mayan Hieroglyphics. D. G. BRINTON. (Publications of the University of Pennsylvania; Series in Philology, Literature and Archæology, Vol. III, No. 2), Boston, 1895, VI, 9-152. pp. 80.

The object of this primer is "with the greatest brevity to supply the learner with the elements necessary for a study of the native hieroglyphic writing of Central America," and Dr. Brinton is of all students of this, the problem of American archæology and linguistics, best qualified to perform such a task, combining as he does a terse and vigorous style with a directness of thought, and an instinctive grasp of the essentials of the subject under discussion, that are not often found among those who have treated of this *cruz* of Americanists, the graphic system of the Mayas and their congeners in ancient Central America. After a brief introduction concerned with the general character of the Mayan hieroglyphics, the manuscripts and the various theories of interpretation, "alphabets" and "keys," that have been put forth from time to

time, since the effort of Bishop Landa, in 1570, the author proceeds to discuss "the mathematical elements" (pp. 18-36)—numeral system, rhetorical and symbolical use of numbers, methods of counting time, the ritual calendar, the astronomical knowledge of the ancient Mayas; "the pictorial elements" (pp. 37-77)—religion and cosmogony, pictorial representations of divinities, quadrupeds, birds, reptiles, occupations and ceremonies; "the graphic elements" (pp. 78-126)—the direction in which the glyphs are to be read, the composition of the glyphs, analysis of various graphic elements, hieroglyphs of the days, months, deities. The fifth and last section of the book is devoted to reproduction of some seventeen specimens of Central American hieroglyphic texts with appropriate comments and explanations. There are three indexes: (1) Index-vocabulary of Maya words; (2) index of authors; (3) general index; and the typography and general get-up of the work are beyond reproach and worthy of the æsthetic subject with which it deals. As an interpreter of these hieroglyphics, Dr. Brinton takes a position intermediate between the German writers, who maintain that they are mainly or wholly ideographic, and the French school (followed by many Americans), who look on them as largely phonetic, holding "that while chiefly ideographic, they are occasionally phonetic"—*ikonomatic* is the term the author has applied to this system of writing, which at times is practically a rebus. The manuscripts or codices of the ancient Mayas, of which four imperfect examples are preserved, the author thinks are much more astronomical in character than even Dr. Förstemann, who has gone furthest hitherto in this interpretation, believes: "they are primarily and essentially records of the motions of the heavenly bodies; and both figures and characters are to be interpreted as referring in the first instance to the sun and moon, the planets and those constellations which are most prominent in the nightly sky in the latitude of Yucatan." This contention, Dr. Brinton ably supports by evidence that can scarcely fail to convince. It is interesting to learn that Dr. Förstemann's explanation of the 24th page of the Dresden Codex, with which Dr. Brinton agrees, indicates "that it was intended to bring the time covered in five revolutions of Venus into relation with the solar years and the ceremonial years, or *tonalamatl* of 260 days; also to set forth the relations between the revolutions of the moon and of Mercury; further to divide the year of Venus into four unequal parts, assigned respectively to the four cardinal points and to four divinities; and finally, to designate to which divinities each of the five Venus-years under consideration should be dedicated." What a wealth of astronomical and mythological ideas these old Americans possessed, we are only now really discovering.

The sections on the religion of the ancient Mayas, their cosmical conceptions and the representations of their gods and goddesses are supremely interesting, and here the author's acute interpretative instinct is seen at its best. Of the 950 figures of deities in the four codices, 638 (more than three-fourths) have been recognized, of which no fewer than 196 are of Itzamna, the long-nosed, snake-tongued god of life and medicine, who has many manifestations. In his exposition of the graphic elements, Dr. Brinton adds not a little to our stock of knowledge, the ingenious exposition of the "drum" signs being noteworthy. The evolution of the "hand" and "eye" signs is also well brought out.

Altogether, this primer is a contribution to the study of American paleography, which does credit to the distinguished Americanist from whom it emanates, and saying this is to pass the best judgment upon the work. Psychologists cannot fail to find in this little volume, concerned as it is with the beginnings of literature and alphabetic writing, many things of profound and lasting interest.

Se vi sono Donne di Genio. G. SERGI. Torino, 1894 (*Estratto dagli Atti della Società Romana di Antropologia*, Vol. I, Fascicolo 20, 1894), pp. 18, 8vo.

As text for his essay, "Are there Women of Genius?" Prof. Sergi puts forward the following statement: "Morphologically and functionally, woman fails to reach the normal male development, remaining generally behind, as if there were a general arrest of development." Woman in many ways presents traits of childhood, which have disappeared in the adult male. The author's conclusion is: "That woman has not genius like man, is easy to demonstrate; yet, it cannot be doubted that there are many women gifted with high intelligence and energy in literary productions and in the fine arts, but such women are not geniuses." Though there are no woman geniuses, Prof. Sergi writes: (1) "That without being a genius, woman may be the mother of geniuses, for these have received superior characters found in her; (2) being of modest or low intelligence she can give birth of offspring of equal value; (3) being without high intelligence she can be the means of transmitting superior faculties by the paternal or atavistic line; (4) the 'genius' of woman when it exists, is rudimentary, latent, and remains so as a sexual fact; and no *milieu* or other favorable factor is ever able to develop it to the degree of male genius; (5) there is sometimes an apparent genius, a superiority over the average, but really a male heredity which develops with physical male characteristics in the same woman, as Lombroso has observed; this is an abnormality, an heredity imperfect by lack of corresponding selection in the secondary sexual characters.

Die Seele des Weibes. Versuch einer Frauen-Psychologie. DR. FERD. MARIA WENDT. Korneuburg, 1892, pp. 130.

Brain anatomy shows no inferiority in the brain of women. The specific gravity of the gray matter in woman's brain is greater than in man's. Her brain is absolutely smaller but relatively larger. The weight of man's brain is to the weight of his body as 1:36. Woman's, 1:35. The nerve tracts are shorter enabling impressions to reach the brain sooner. This accounts for the greater mental activity and quickness of women and small men. On the other hand, there is less oxygen in the blood in the case of woman, not favorable to mental activity. This is not compensated for by frequency of heart beat and respiration. To the advantage of woman is her greater sensibility. The threshold, both as regards stimulus and time, is lower. Her finer sensibility is shown particularly in the sense of touch but also in temperature, taste, smell, sight and hearing. Woman has greater receptivity and more rapid and accurate perception. The rapidity of her perceptions and representations is shown in her greater power and rapidity of speech. Her memory is better, particularly for colors, tastes, caresses and for concrete pleasant and unpleasant experiences. She excels in imaginative power and in phantasy, and it seems a pity, says the author, that she has not excelled in music, painting, poetry, and in the other arts, depending upon this faculty. The book is childlike throughout and of no psychological value.

X.

La Psychologie des Sexes et ses Fondemens Physiologiques. ALFRED FOUILLEE. *Revue des Deux Mondes*. Vol. 3, 1893, pp. 33.

It has been said that minds have no sex. This might be true if we were pure spirits, but in our present condition our characters receive the impress of our organisms. In psychological problems

concerning the moral and social relations of the sexes, biological considerations have not been sufficiently regarded, but biologists are now introducing elements of great value into the study of the sexes, by an exact characterization of the physical and mental traits of each sex. If it is true that morality and science should follow nature, the origin, characteristics and purpose of the sexes in the evolution of life cannot be disregarded. To entirely obliterate the differences of sex would be, as Geddes and Thompson have said, to "begin evolution upon a new basis."

Since the earliest antiquity philosophers have maintained that woman is an example of arrested development. This idea has been proved false by recent science. It is now shown that the embryo receives a mathematically equal part of maternal and paternal substance, and that the sex is determined by the amount of nourishment provided. Insufficient nourishment tends to produce males, and more favorable conditions of nutrition females, so that the feminine sex, far from being the result of arrested development demands the most favorable conditions of nourishment to determine it. M. Armand Sabbatier had already found that the characteristics of the female are concentration, unification, cohesion,—of the male, division and dispersion. Carrying this method further we observe that the female represents solidarity, inwardness, dependence; the male, differentiation, outwardness, independence. The female represents receptivity, economy; the male, motion, activity, expenditure. In the female the temperature is lower and the consumption of vitality less. The female is calm, tender, altruistic; the male, restless, explosive, egoistic. The female excels in finesse, cunning. It is her forte to wait, observe, divine. Her brain is more refined. The male excels in intellectual effort, in attention, in penetration. His brain is larger. The female represents beauty, the æsthetic element, passivity, impressionability. Her temperament is phlegmatic or lymphatic. The male represents force, the dynamic element. His temperament is choleric or bilious. The female is more apt in particulars, details; the male, in generalization and abstraction. The female is intuitive and, when not ruled by feeling, sees more justly. The male is deductive, analytic, and sees farther and deeper. With the female the association of ideas is in space, by contiguity; with the male, in time, by causality. The female has a better memory, is more imaginative, more positive and practical, not given to Utopias, is reserved, circumspect and prudent, is more conservative and has more common sense.

As we rise in the scale of animal life these sexual differences become more marked. Darwin and Spenser have endeavored to explain them by natural selection, but that explanation is incomplete. An internal, not an external determinism fixes the primary characteristics of the two sexes. The explanation of moral differences should likewise be sought in fundamental organic differences. These result in woman in the complexity and preponderance of the affections. Some have cited the conjugal and maternal love of woman as an evidence of inferiority, because the same is found among the lower animals. But we might apply this mode of reasoning to masculine superiorities, as for example, to courage. The fact that courage points back to early stages of evolution is not a reason for depreciating it.

In considering the defects in woman's intelligence it is difficult to determine those that belong to her nature and those that are the result of inferior instruction during past ages. It is certainly evident that reform is needed in the instruction provided for women,

and her economic and judicial advantages are far from what they should be. Identity of function in man and woman in society is impossible but there should be an equivalence of duties, better regulated by law. To find the just balance that shall insure an equality between duties and rights in the family and in social life is one of the great problems of the future.

Touillée's work is certainly the best that has recently been done on this subject. Nowhere is the want of accurate detailed work in psychology so much shown as in the scattered efforts that have been made thus far to outline a psychology of woman.

Y.

Ueber die Frauenemancipation. Von DR. GUSTAV TEICHMÜLLER. Dorpat, 1877, pp. 95.

The term "Emancipation of Woman" implies her present condition to be one of slavery. The facts warrant this implication. Is this a necessity such as the partial subjection of children is admitted to be? Is the difference between the sexes of such a character as to warrant the assumption that the final aim of woman differs from that of man? All views with regard to woman can be classed under three heads. The first view is that of Aristotle which assumes that the actual condition of woman realizes the design of nature. The second is that of Plato who holds that her individual existence as a moral and intellectual being entitles her to the enjoyment of a like freedom with man. Important as are the reproductive activities they are only a means to the continuance of the race, and can never be to either man or woman an end in themselves. The final end of every human being must be the development of his mental powers, in the possession of which high gifts women are equal sharers with men. To this sound philosophic view the third, that imposed by social necessity sets its seal. The only adequate alleviation of the miseries resultant upon overpopulation is for the state to fit everyone of its children, regardless of sex, to earn a livelihood.

C. H. S.

III.—MISCELLANEOUS.

Entwurf einer ontologischen Begründung des Seinsollen. Von GUSTAV ENGEL. Berlin, Wm. Herty, 1894, pp. 212.

The author is a vigorous philosophical writer, who for nearly forty years has been thinking Hegel's thoughts into both more condensed and more modern form. This is his masterpiece. It is Hegelism complete, and more or less atoned with Darwinism. Duty, being and evolution are one. They comprise all psychologically possible forms of a beginning. What *ought to be* is traced from the lower spheres of number, quantity, time and space to art and morals, till in the union of the inner and outer, conduct and religion, the problem of the possibility of the higher world is answered. The quintessence of Hegelism has never been more succinctly stated.

I Misteri della scrittura. Lettura tenuta al Circolo sociale Trevigiano il 16 Aprile, 1893. GIUSEPPE STUCCHI. Treviso, 1890, pp. 92, 12mo.

In this interesting little book, the author, who is professor of philosophy in the Liceo di Treviso, treats of the "mysteries of writing," graphology—that universal belief in the existence of some connection between the writing of a person and his character,

a belief so common and so wide-spread that it cannot be entirely groundless. After noticing very briefly Severino's "*Vaticinator*," Descuret's "*Medicina delle passioni*" (in which is to be found a study of the writing of Silvio Pellico by the Abbé Flandrin), Henze's "*Chirogrammato mancia*," Delestre's "*Mystères de l'écriture*," and the later works of Michon, "*Crepieux-Jasmin*," "*Deschamp*," etc., Prof. Stucchi remarks: "Graphology, which ought to be a most valuable auxiliary of psychology, has, like this, its essential basis in a third science, physiology;" and proceeds to outline the nature and practice of graphology. His conclusions are: (1) That graphology, like any other science, has a theoretical and a practical part, and from the exact and sure application of theoretical principles comes, with long and patient exercise, special ability; (2) that in order to establish an exact correlation between certain graphic signs and the moral and intellectual qualities of one's self and of others, a better knowledge of one's self and of others is necessary in order to avoid falling into grievous error; (3) that not all the graphic manifestations have the same value for graphological inquiry; (4) that a single writing is insufficient to reveal the nature of a given person.

A. F. C.

Schmerz und Temperaturempfindung. Von PROF. DR. Z. OPPENHEIMER. Berlin, 1893, pp. 128.

This thoughtful and important paper takes a step beyond Bouller, Dumont, Mantegazza, Vel, and scores of other writers on pain, a subject which has been under investigation at Clark University the past year. Pain affects the course of disease, and, indeed, fills the history of medicine, which wars on it. It is a degree, not a kind of sensation. The fact that saponin kills touch and not pain, while chloroform kills pain, but not touch, shows that their centres or conductive fibres or both are different. Pain is not the maximal sensation a sense-organ, but the most intense sensation which follows the strongest stimulus in the vaso-motor nerves. Besides, the interruption of pain conductivity and of vascular innervation, the increase and reduction of the sensations of temperature, have been noted in all fully recorded cases of syringomyelia, or degeneration of the posterior horn of the spinal cord. Touch nerves do not pass here and have no known connection with the horn, hence, so long as temperature sensations were thought to be mediated by tactile nerves, this was inexplicable. Temperature sensations are unique in being composed of simultaneous action of sympathetic and of tactile nerves. What has been called the sympatheticus is composed of two quite distinct groups of fibres, viz., the splanchnicus and the sympathetic system proper. The latter is peculiar among all nerves in that it has centripetal and centrifugal conductivity by its connection with anterior and posterior roots, and also by forking at the peripheral end a second arrangement for centripetal and centrifugal conductivity is provided, of which the latter innervates the nerves, and the former mediates the stimuli which proceed from the tissues. A constant excitation goes from the anterior roots to preserve the vascular tonus, which may be inhibited by an opposite pain current from the periphery, causing relaxation of tonus and hyperæmia.

A Review of Evolutionary Ethics. By C. M. WILLIAMS. Macmillan, New York, 1893, pp. 581.

The first 268 pages are devoted to well made digests of thirteen leading writers on evolutionary ethics, beginning with Darwin and embracing Wallace, Haeckel, Spencer, Fiske, Ralph, Barrett,

Stevens, Carneri, Höfding, Gizycki, Alexander and Ree. Digests are hard to make, but it is indispensable, in these days of rapidly accumulating literature, that they shall be made, and made systematically and thoroughly. Mr. Williams has acquitted himself pretty well here. Part II. is his own and is devoted to end, will, relations of thought, feeling and will, egoism and altruism, conscience, progress, results, and the ideal, and the way of its attainment. Christianity is defended as a "comforting belief." The discussions are practical and treat of such themes as the labor question, luxury, machinery, Bellamy, education, the status of women, rights of universities, capital punishment, altruism, change of heart, slavery, sacrifice, golden age, democracy, habit, health, want of rest, pleasure, end, law, etc. The length of the discussion is atoned for by frequent summaries. The highest joy of human association is the love of noble characters. The final destruction of the race need not trouble us. A far greater source of present pain is the loss of faith in personal immortality. It leaves death a harder sorrow, but it lends life a new meaning. The good we strive for lies here. We must, therefore, draw closer in sympathy and by mutual kindness render loss less bitter. We must bow to the inevitable and strive to "join the choir invisible of those immortal dead who live again in minds made better by their presence," to scorn the "miserable aims that end with self, in thoughts sublime that pierce the night like stars," and thus enkindle generous ardor, feed pure love, and make the "music which is the gladness of the world."

Psychologie du Militaire Professionnel. Par A. HARMON. Paris, 1894, pp. 216.

This is one of the "social psychology studies," and has excited great interest and opposition. The author's main theme is that armies are a source of crime, and he has striven to give us a work of science and not a collection of scandals. He finds that army life depresses mentality, breeds contempt of human life and physical suffering, causes brutality and grossness, both within and without the profession, and provokes sexuality and legal criminality. Physical analgesia, moral anæsthesia, the fact that all is supported by an *esprit du corps*, the distaste for useful labor, the substitution of brute force for respect for right, — these cause the demoralization, misery, alienation and suicide which statistics show to be so prevalent among military men.

Apperception and the Movement of Attention. G. F. STOUT. Mind, Vol. XVI, 1891.

In this analysis of the thinking process, Stout uses the term "Apperception" in the Herbartian sense. Attention is a motor-process, a muscular action which cannot be sharply marked off from that which produces physical change in external things. It involves actual movement, muscular strain, or at least motor impulse. It is not an occasional act. In the clearness and strength of presentations which successively become salient, there is merely a difference of degree; but between the salient presentation at any moment and the out-zone constituents of mind, there is an unbridged chasm. This unique salience must be due to a specific process which is called attention.

Mental elements, like social elements, group into systems. So long as the system lasts, it prevents its elements from acting in any other system or independently. It may break up and set its components free, or may unite with other systems and thereby

limit the action of each element. "The process by which a mental system incorporates, or tends to incorporate a new element," is apperception. In this synthesis, attention aids apperception by focussing a presentation until the apperipient system has succeeded or failed in assimilating it. Such assistance is needful where the complexity of an apperipient group or the novelty of a presentation retards incorporation; it is unnecessary where incorporation has grown easy.

In its activity, the apperipient system is not isolated; it excites other systems and these tend in turn to act (Coöperation). At the same time, it weakens all those groups which are not capable of combining with it in the same systematic activity (Competition). The more a system coöperates with others, the less it is able to compete with them. A presentation which may be apperceived by different systems, is grasped by the strongest, *i. e.*, by the one whose action has been most recent and intense, has been freshened by repose or stimulated by organic sensation. Intrinsically, the promptness of a system to apperceive depends upon its symmetrical organization, its comprehensiveness, the cohesiveness and sense-character of its parts.

This strength is tested in the *conflict* which arises when "one system in assimilating a new element tends to wrest it from its preformed connection with another." If the attempt succeed, the result is *positive* apperception; if it fail, *negative* apperception; and at times the issue may remain in suspense, as when we are left hopelessly in doubt.

To illustrate this normal inter-action by contrast: In hypnotic suggestibility, a dominant system excited by the hypnotizer, and exercising unlimited tyranny, prevents that mutual competition and coöperation which would correct or dispel hallucination.

Attention is fixed upon a particular presentation by the *feeling* which accompanies apperception. This feeling plus apperception, constitutes *interest*. Among several presentations brought up by sensory impression or association, that one excites interest and is selected by attention which is congruent with the most excitable apperceptive group. To this congruence, the likelihood of apperception is directly proportioned; the likelihood of accompanying attention, inversely proportioned; for when the presentation is well conformed to the group, it glides into place without a ripple of attention.

The presentations successively attended to form the train of ideas. As distinguished from this, the *train of thought* implies that each idea be apperceived by the same persistently dominant system, and that the relation linking each idea to its predecessor, form also a source of the interest through which it attracts attention. This distinction rests upon the organization of mental systems. Thought involves the activity of *proportional* systems, *i. e.*, of systems whose constituent elements are united according to an analogous plan or type, and which apperceive objects otherwise diverse, "merely because they agree in being capable of entering into certain relations."

There is a consequent distinction between reproduction by simple association and *proportional* reproduction. In this latter, it is not the similarity or other special character of the presentations *in se* that determines the revival, it is the analogy of constituent relations. (Let $\frac{a^n}{b^n} = \frac{a}{b}$; then a^n will call up, not a or b , but b^n ;—though it resemble a .)

A final characteristic of thinking is the reversion of attention to previous links in the train of ideas, giving rise to a modified repetition of it, and avoiding that conflict which previously hindered the incorporation of a presentation in an apperceptive system.

Le Sentiment et la Pensée. Par ANDRÉ GODFERNAUX. Paris, 1894, pp. 224.

This essay in comparative and experimental psychology is dedicated to Ribot and Dr. Magnan. Five chapters are first given to the description of mania, melancholy, hypochondria, ecstasy and chronic delirium. Normal psychology is treated from the standpoint of excitation or depression, and the emotions and association of ideas are treated as muscular tendencies and coordinations. The general conclusion is that the work by which the effort of a tendency, while yet vague and undecided, to specialize itself into more or less complex groups of motor phenomena, corresponds, in the last case of definite muscular coordinations, to the work by which an emotion takes concrete form and creates a definite synthesis of the elements of consciousness.

Lowell Lectures on the Ascent of Man. By HENRY DRUMMOND. New York, T. Potts & Co., 1894, pp. 346.

The last few lectures of this interesting course are omitted, and instead is a long introduction of fifty-six pages. Probably there was never a book that admitted being condensed so completely into a few sentences. Evolution is a grand drama approaching its last act, man. This is the age of the evolution of evolution. The beginning must be interpreted from the end. Darwin too much ignored man. The struggle for the survival of others began with the first care for the egg. The seventy vestigial structures which Weismann enumerates in man show his evolution from lower forms. The arrest of the body came with tools. Now this is a psychical universe. Soul growth begins with feelings which we share with the lower animals. Old age and death show traces of devolution. The evolution of motherhood stands for altruism and love. The father comes later and stands for justice. The family was very slowly unfolded, and is the root of all sexual institutions. The world's history is a love story. Nutrition and reproduction are the roots of selfishness and unselfishness respectively. All is progressive. God does not live in gaps, and love is the consummate blossom of all evolutionary processes. It is the old Pauline charity. The book is a pleasant and very popular summary of the world processes from the atom to the saint. On the whole very liberal and progressive, and to be most warmly commended to all who still feel the old sense of opposition between science and religion, all trace of which the author himself has, however, by no means escaped.

Basal Concepts in Philosophy. An Inquiry into Being, Non-Being and Becoming. By ALEX. T. ORMOND, PH. D., Professor of Philosophy in Princeton University. New York, 1894, pp. 308.

Contemporary thought is chiefly marked by its weakness in respect to fundamental philosophical conceptions. This causes sensationalism in psychology and phenomenism in philosophy, and hence comes agnosticism on one hand and monistic pantheism on the other. Intermediate between these the author would ground the world of reality in an Absolute, possessed of supreme intelli-

gence, goodness and love. The author acknowledges "the great debt I owe to my honored teacher, the venerable McCosh, to the spirit of whose realistic philosophy I hope my own work will be found loyal." The author states that he is also indebted to Plato, Aristotle and others. The author is essentially right in his main positions, but only in a sense like that in which the Old Testament prophets of Christ were Christian. He dimly sees the re-revelation of his own doctrines in the newer directions of science, but says very little that might not have been said fifty years ago. The book is an honest, earnest, old-fashioned plea for old doctrines on good old grounds.

The Synthesis of Mind, the Method of a Working Psychology, by Corydon Ford (J. V. Sheehan, Ann Arbor, Mich., 58 pp. 8vo.), is a book full of wild and unintelligible verbiage, and absolutely without value, except as a study in abnormal psychology. H. A. A.

The Reality of the Self, by W. L. Courtney, Esq., M. A., LL. D. (Being a paper read before the Victoria Institute, 25 pp. 8vo.) is a very brief and superficial recapitulation of some of the spiritualistic arguments. It is modest and clear, but neither the paper itself nor the discussion following contains any contribution to rational psychology. H. A. A.

To the growing list of psychological periodicals is to be added a psychological annual—*L'Année Psychologique*—under the management of the distinguished directors of the Psychological Laboratory of the Sorbonne, MM. Beaunis and Binet. With these gentlemen are associated as collaborators, Ribot (College de France), Flournoy (Geneva), Delabarre (Brown University), Weeks (Harvard), V. Henri (Leipzig), Philippe (Paris), Courtier (Paris) and Bourdon (Lille). The four parts of the annual will be devoted respectively to: 1. A full account with abstracts, tables and diagrams of all the important psychological literature of the year just past. 2. A bibliographical index of the literature of 1894—covering more than twelve hundred titles. 3. Original contributions by Binet and Passy, Binet and Henri, Flournoy, Delabarre and Weeks—Prof. Delabarre's paper being an extensive description of the present state of psychology in America. 4. News and Notes of psychological interest. The regular price of the volume, the risk and cost of which are born by MM. Beaunis and Binet without expectation of profit, will be 10 fr. The work will be ready in March and may be had from F. Alcan, 108 Boulevard St-Germain, Paris; or from M. A. Binet, à la Sorbonne, Paris. The names of those in charge of the new annual assure both its character and success.

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